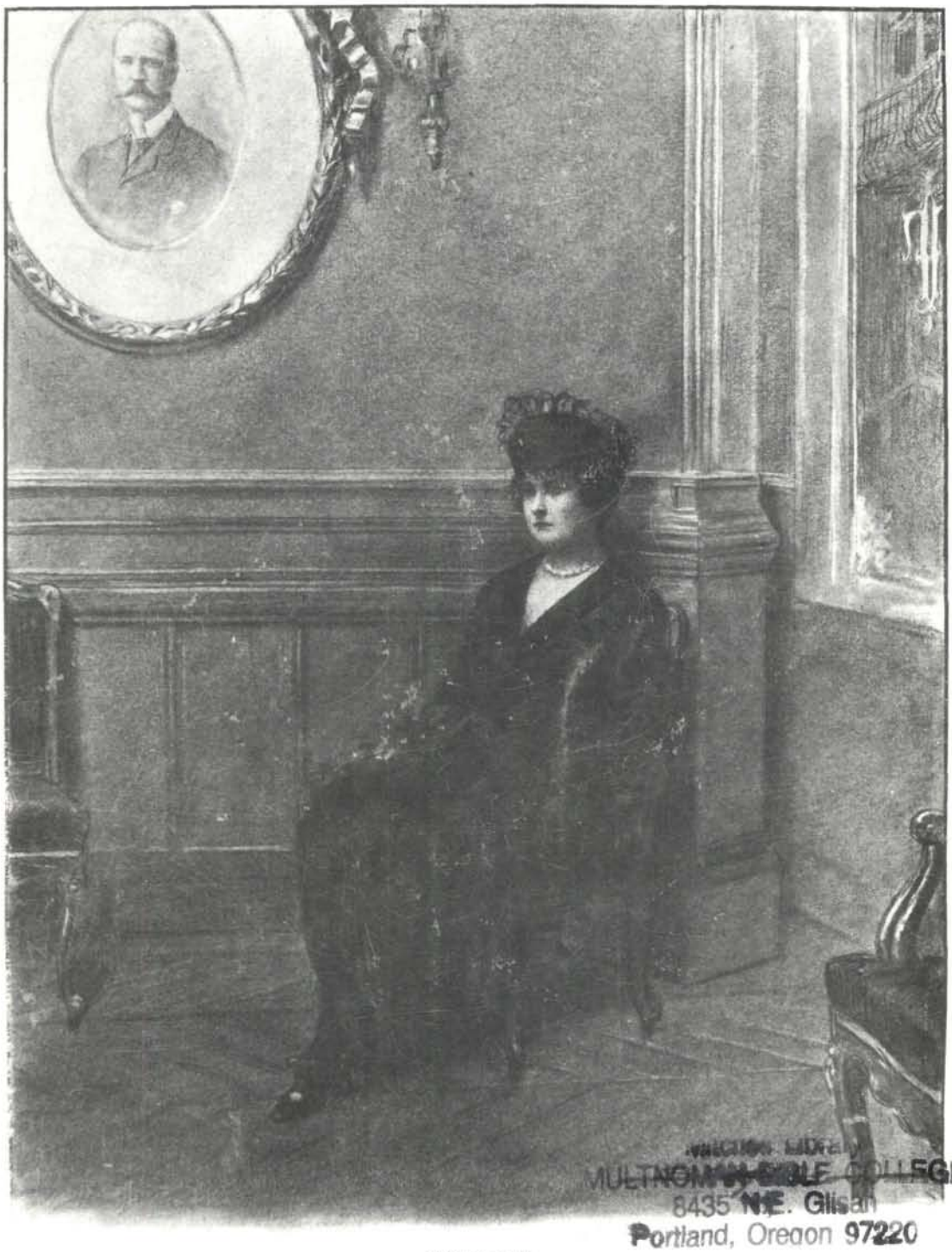
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The American Historical Review

AMERICAN HISTORICAL ASSOCIATION



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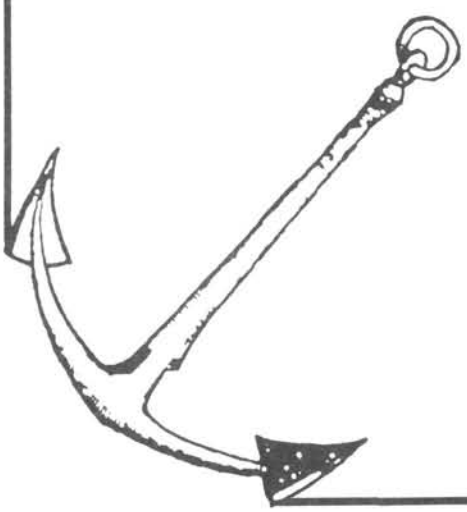
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In This Issue

The Presidential Address by **Natalie Zemon Davis**. Drawing on the ideas of Ernst Kantorowicz and Marc Bloch, Davis explores the "two bodies" of history: the lives and contributions of individual historians and the corpus of history writing as a whole, a body that Davis defines as the product of the interactions of working historians with one another, their challenges to their mentors, and their responses to their students. Each age of the second "body" is illustrated with a characteristic picture, making this the first time that a Presidential Address has been accompanied by illustrations. (The text printed here is an expanded version of that read at the national convention in December.)

Edward Berenson follows the example of Natalie Davis and others who have pioneered the use of a single event or life story as a focus for drawing larger implications about social and political developments. Berenson concentrates on a sensational murder case in early twentieth-century France, the Caillaux Affair, in which the wife of a former prime minister killed an editor who threatened to expose her role in the break-up of her and her husband's previous marriages. The case became the center of a broad public debate on divorce law and, by extension, on the sanctity of the family and the security of the nation.

The "Asiatic mode of production" is one of the legacies of Marxist thought that has not received much elaboration by Western scholars. Yet for understandable reasons it evoked more interest among writers in Russia, China, and Japan who were struggling to reconceptualize the periodization of their histories within a Marxist theoretical framework. **Joshua A. Fogel** offers the first comparative examination of the debates over the Asiatic mode of production that occurred in these three societies prior to World War II. Fogel points out that not only did the debates exhibit specific attributes in each society and become subject to the pressures of local political programs but that writers in each society exerted important influences on the debates transpiring in the others.

By skillful use of a series of papal bulls lying neglected in the archives of Barcelona, **Robert I. Burns** has discovered that what was thought to be no more than a small revolt at the close of King Jaume's crusader conquest of Islamic Valencia in the mid-thirteenth century was in reality a major "lost" war in the Muslim-Christian struggle for Valencia. The papal bulls make clear that the convocation of a full crusade with accompanying Holy Land indulgences, special funding appeals, and actions against King Jaume's Christian enemies was required before the Islamic counterthrust under al-Azraq could be checked and turned back. The story has implications for crusade history generally, for the history of Spain, and for literary history, as it resolves a mystery in the structure of Jaume's memoirs.

The final contribution, a research note by **Randolph B. Campbell** on slave hiring in Texas, illuminates the demographic and economic characteristics of the hirers and demonstrates that over 40 percent of those who hired slaves in the years 1848 to 1862 were not slaveholders at the time they employed bondsmen. This finding suggests that far more Texans were directly involved with slavery than is evident in the records of slave ownership. The system of slave hiring may therefore have increased the popular support for slavery well beyond the limits indicated by commonly cited statistics on slave ownership.

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History's Two Bodies

NATALIE ZEMON DAVIS

HISTORIANS WILL CATCH IN MY TITLE a reworking of *The King's Two Bodies*, the title given by Ernst Kantorowicz to his great study in medieval and Renaissance political theology. Kantorowicz's curiosity had been aroused by Frederic Maitland's essays on the English "corporation sole": how intriguing it was that sixteenth-century lawyers had invented a corporation with just one person in it and had talked of the king as having "a body natural and a body politic together . . . in one person," the one "subject to all Infirmities that come by Nature or Accident," the other "utterly void of Infancy, and old Age." Deepened by Kantorowicz's exploration of the long Christian past behind this "mystic fiction" and by more recent scholarship on royal funerals and other ceremonial, the concept of the king's two bodies generates questions about power and about succession. How is the tension maintained between the decisions and acts of the ruler's natural body and his eternal body politic? How does the natural king envisage political continuity after his death, and how is the transition to his successor realized? How are these processes affected not only by changing historical contexts but by the circumstance of the ruler's person—if, for instance, the ruler is from a new royal house or the ruler is a queen?[1]

The categories of the king's two bodies have been so fruitful for analyzing the responsibilities and potentialities of office that historians have gone on to apply them to non-dynastic cases, such as the Catholic papacy and the American presidency.[2] I would like to extend them even further to the case of a scholarly

[1] Frederic William Maitland, *Selected Essays*, H. D. Hazeltine, G. Lapsley, P. H. Winfield, eds. (Cambridge, 1936), chaps. 1–2. Ernst H. Kantorowicz, *The King's Two Bodies: A Study in Mediaeval Political Theology* (Princeton, N.J., 1957); Ralph Giesey, *The Royal Funeral Ceremony in Renaissance France* (Geneva, 1960); Sarah Hanley, *The Lit de Justice of the Kings of France: Constitutional Ideology in Legend, Ritual, and Discourse* (Princeton, N.J., 1983); Richard Jackson, *Vive le Roi! A History of the French Coronation from Charles V to Charles X* (Chapel Hill, N.C., 1984); Lawrence Bryant, *The King and the City in the Parisian Royal Entry Ceremony: Politics, Ritual, and Art in the Renaissance* (Geneva, 1986); Marie Axton, *The Queen's Two Bodies: Drama and the Elizabethan Succession* (London, 1977); Claire R. Sherman, "The Queen in Charles V's 'Coronation Book': Jeanne de Bourbon and the 'Ordo ad Reginam Benedicendam,'" *Viator*, 8 (1977): 255–97. For new bibliography on this topic, see the recently established newsletter, *Majestas: Rulership/Souveraineté/Herrschertum*.

[2] Peter Burke, "Sacred Rulers, Royal Priests: Rituals of the Early Modern Popes," in *The Historical Anthropology of Early Modern Italy: Essays on Perception and Communication* (Cambridge, 1987), 168–82; Laurie Nussdorfer, "The Vacant See: Ritual and Protest in Early Modern Rome," *Sixteenth Century Journal*, 18 (Summer 1987): 173–89. Michael Rogin, "The King's Two Bodies: Lincoln, Wilson, Nixon, and Presidential Self-Sacrifice," in *"Ronald Reagan," The Movie and Other Episodes in Political Demonology* (Berkeley, Calif., 1987), 1–43.

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field, our field of history, and democratize them beyond the persons of mere officers to ordinary practitioners. Kantorowicz himself prepared the way for such a leap when he talked about Vergil crowning Dante with the eternal dignity of Humanitas;[3] if "Man" can have two bodies, at least, in thought, why can't History? I want to consider how historians have conceptualized the body of historical knowledge and have placed their own life's work within it. I want to see how they maintained the rightful tension within their bosoms between the field that endures and their own brief embodiment of its claims, a tension usually expressed in relation to other historians, past, present, and future.[4] Several issues can be in play here: property, reputation, riches, politics, sometimes prophecy, sometimes reform. When we debate what the subjects and methods of history should be, we are usually debating at the same time what the shape of the historical community should be and where we stand in it. So let us consider how history's two bodies were mediated by five historians in different settings, one from the sixteenth century, two from the eighteenth century, and two from the twentieth century.

OUTSIDE THE AREA OF LAW, of the *Corpus juris civilis* and the *Corpus juris canonicus*, sixteenth-century scholars rarely employed the word "body" to refer to all the existing findings and texts in a given field. The French royal historiographer felt somewhat experimental in 1587 when, after talking of the body politic, he said that he was "reducing" many chronicles and annals into a universal history "as in one body or harmony." The wider usage of the "body of knowledge" would await the republic of letters of the eighteenth century; in the meanwhile, Renaissance historians who wanted to write about the state of their subject entitled their books, as did Jean Bodin, *Method for the Easy Comprehension of History* or, as did La Popelinière, *L'Histoire des Histoires*.[5] History was a "discipline" but not one taught as such in the schools. The books in history's "treasure house" did not have listeners ("discipuli") in a classroom or become printed university textbooks but had readers in the wider world.[6] History was an "art"—not one of the seven arts that made up the Trivium and Quadrivium, but, like poetry, drama, and song, one of the nine arts to which the Greek world had given a Muse.

These goddesses, after a shadowy existence during the medieval period, burst forth with Apollo at Parnassus in Renaissance dictionaries and images to preside

[3] Kantorowicz, *King's Two Bodies*, 491–95.

[4] Some of these questions have been broached in a splendid essay by Michael Kammen, "Vanitas and the Historian's Vocation," *Reviews in American History*, 10 (1982): 1–27.

[5] Nicolas Vignier, *La Bibliotheque Historiale . . . Contenant la disposition et concordance des temps, des histoires, et des historiographes* (Paris, 1587), f. a iii^{r}. Jean Bodin, *Methodus ad facilem historiarum cognitionem* (Paris, 1566); Lancelot Voisin, sieur de La Popelinière, *L'Histoire des histoires, avec l'Idée de l'Histoire accomplie* (Paris, 1599). On sixteenth-century French historiography, see Julian Franklin, *Jean Bodin and the Sixteenth-Century Revolution in the Methodology of Law and History* (New York, 1963); George Huppert, *The Idea of Perfect History: Historical Erudition and Historical Philosophy in Renaissance France* (Urbana, Ill., 1970), Donald R. Kelley, *Foundations of Modern Historical Scholarship: Language, Law, and History in the French Renaissance* (New York, 1970); and Anthony Grafton, "From *De Die Natali* to *De Emendatione Temporum*: The Origins and Setting of Scaliger's Chronology," *Journal of the Warburg and Courtauld Institutes*, 48 (1985): 100–43.

[6] Bodin, *Methodus*, 4: "historiae thesaurus." On what was going on in the Renaissance classroom, see Anthony Grafton and Lisa Jardine, *From Humanism to the Humanities* (London, 1986).

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ILLUSTRATION 1: *Clio*, an engraving by Hendrik Goltzius, 1592. From Hendrik Goltzius, *The Complete Engravings and Woodcuts*, Walter L. Strauss, ed. (New York: Abaris Books, 1977). Courtesy of Marquand Art Library, Princeton University.

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over the high disciplines where intelligence, memory, and inspiration were required. The graceful Muse embodied eternally in her female form the arts that men practiced. They could not *be* the Muse; she favored them from without and represented their activities and the qualities to which they must aspire. Clio, with her trumpet and laurel, sang of glorious deeds and events of the past and promised renown to the historians themselves. She also had her instruments of work: her books, pen, and tablet. In Hendrik Goltzius' engraving of 1592 (Illustration 1), Clio looks down at what she has written with a faint smile, perhaps ironic, certainly detached. From this picture, it is only a short step to some Renaissance representations of History as a winged woman writing, her white garb signifying that she bears witness to truth as well as to renown.[7]

Etienne Pasquier did not represent himself with laurel in the portrait of the author that accompanied the edition of his historical studies in 1607, *Les Recherches de la France*; the face that peers out at us is capped by his proper lawyer's bonnet. Even in his poetic works, he distanced himself from the "fury" of a Tasso and told readers he did not much care how his poems fared in the risky course of immortality. So, too, years before in publishing Book I of his *Recherches*, he said it was most often the "chance of the moment, like a blind person playing cards" that determined the rewards won by a book, not its actual value.[8] Nonetheless, credit was important to Pasquier, if not renown, and credit in connection with just those issues raised by the pioneering features of his *Recherches*.

For Pasquier, history was in no state to be "reduced to one body or harmony" (as in the royal historiographer's *Bibliotheque historiale*) or to be organized into a comprehensive philosophy about climate and ethnic character (as in Bodin's *Method*).[9] The historical sources themselves were what needed attention, no longer

[7] De Witt T. Starnes and Ernest W. Talbert, *Classical Myth and Legend in Renaissance Dictionaries* (Chapel Hill, N.C., 1955), 90–99; Elisabeth Schröter, *Die Ikonographie des Themas Parnass vor Raffael* (Hildesheim, 1977), includes full pictorial evidence and bibliography. On the gender issue in representation of the Muses, see Marina Warner, *Monuments and Maidens: The Allegory of the Female Form* (London, 1985), chaps. 4, 9 and 233–36. Descriptions of Clio in Ambrosius Calepinus, *Lexicon* (Lyon, 1538), 368, 1274–75; Robert Estienne, *Thesaurus Linguae Latinae Editio Nova*, 2 vols. (London, 1734–35), 1: "Clio"; Charles Estienne, *Dictionarium Historicum, Geographicum, Poeticum* (Geneva, 1660), 712; Vincenzo Cartari, *Les Images des Dieux*, Antoine du Verdier, trans. (Lyon, 1624), 65–66; Cesare Ripa, *Iconologia* (Padua, 1611), 368. Hendrik Goltzius, *The Complete Engravings and Woodcuts*, Walter L. Strauss, ed., 2 vols. (New York, 1977), 2: 542–43. On Clio's attributes and on History as a woman writing, see Guy de Tervarent, *Attributs et symboles dans l'art profane, 1450–1600* (Geneva, 1958–59), 139, 168, 281, 387.

[8] *Les Recherches de la France d'Estienne Pasquier, Reveuës et augmentees d'un Livre, et de plusieurs Chapitres par le mesme Autheur* (Paris, 1607). The picture had first appeared in the 1586 edition of his *Lettres*. The posthumous 1621 edition of *Les Recherches*, published in Paris by Jean Petit-Pas, has a sober portrait of Pasquier, remembered as he looked not long before his death at age eighty-seven. *Les Ieus poetiques d'Estienne Pasquier* (Paris, 1610), Au lecteur, Tours, 19 January 1592. The quotation is from the 1560 edition of the *Recherches*, which included Book I only: "estant le hazard du temps, comme l'aveugle és blanques, distributeur des benefices que reçoivent les Livres, et non le plus souvent leur valeur" (*Les Recherches*, Book I, chap. 1). For a similar image of Fame distributing her rewards at random, see the discussion of Chaucer's *House of Fame* in Leo Braudy's excellent book, *The Frenzy of Renown: Fame and Its History* (New York and Oxford, 1986), 241.

[9] On Pasquier's historical methods and contribution, see George Huppert, "Naissance de l'histoire en France: Les *Recherches* d'Estienne Pasquier," *Annales: Economies, sociétés, civilisations*, 23 (1968): 69–105; Kelley, *Foundations*, chap. 10; and Orest Ranum, *Artisans of Glory: Writers and Historical Thought in Seventeenth-Century France* (Chapel Hill, N.C., 1980), 75–82. On Pasquier's life and works, see D.

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to be accepted at face value but to be judged by internal and external evidence and understood in terms of the period in which they were produced. As legal humanists were taking apart the laws in the *Corpus juris civilis* to get at their original meaning, so he would sift the claims of past historians and authors, both those that "by the long passage of time have insinuated themselves among us and are reputed to be true" and those that were challenged on every side. What real evidence was there for the Trojan origin of the Franks or, indeed, for the ancient beginnings of any nation? How to assess the differing theories about the origins of fiefs: Roman? Gallic? Frankish? an overlay of different institutions?

Pasquier argued that one had to learn to read known texts for their contradictions and silences and to find new texts close to the events "as guarantees." On gypsies and on tennis games in the fifteenth century, Pasquier could quote from "an old journal, fallen into my hands, written on paper by a Paris Theologian, who was careful to collect all the things he saw"; on Jeanne d'Arc, he could cite not only copies of her trial in two libraries but the original record itself, with all its seals and signatures, in his possession for four whole years.[10] The resulting *Recherches* were not organized in a single chronological narrative but in Books, each held together by a common theme—political, ecclesiastical, literary, or cultural. Like Montaigne with his *Essais*, Pasquier kept revising and adding to them after his first edition of 1560, a project continuously in motion over the years.[11]

How strong did Pasquier make his own claims to truth? Was History incarnate in the disjoint Books of the *Recherches*? His tone varied from the absolute certitude with which he condemned the Jesuits and their cabals against the Gallican church ("I [write thus] not for hate vowed against them . . . but for the love I bear my country") to the balanced weighing of the probabilities for competing interpretations with good "guarantees" behind them. At his most vehement, Pasquier could still stand back and reflect upon himself: for instance, he knew he sounded as if he were praising "our old Gauls" too much, but he did not mind so long as everything he said "conformed to the truth" and shook up accepted views.[12]

But Pasquier had a grievance against certain historians of his day. It stemmed from his efforts to redefine his relations to authors and sources from the past. He cited and quoted them at length in French translation or in Latin and French both—at length, not by our standards but by those of sixteenth-century readers of history books. Some found this practice lacking in grace: "They said that most of those who have taught us to write history simply distilled from antiquity all they wished to communicate to people, without amusing themselves with such confirmation [by references and quotations], which was more in keeping with the shadow of the School Room than with the light of History [of History's Muse?—

Thickett, *Estienne Pasquier (1529–1615): The Versatile Barrister of Sixteenth-Century France* (London and New York, 1979).

[10] Pasquier, *Les Recherches* (1607), Book I, chap. 3: 23, chap. 14: 64–66; Book II, chap. 13: 196–204; Book IV, chap. 13: 588–89, chap. 17: 606–08; Book V, chap. 8: 709.

[11] Pasquier, *Les Recherches* (1607), Book I, introduction: 3–4. On the different editions of *Les Recherches*, see D. Thickett, *Bibliographie des oeuvres d'Estienne Pasquier* (Geneva, 1956), 32–44.

[12] Pasquier, *Les Recherches* (1607), Book III, chap. 38: 530; Book II, chap. 13: 198; Book I, chap. 3: 22–23.

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NZD]. Time refines works like gold: what today lacks some credence will tomorrow authorize itself, as has happened with the writings of ancient historians." Others thought it a wise policy to confirm little-known antiquities by quotations, but was not Pasquier carrying it to "superstitious excess"?[13]

Pasquier was aware of the costs of quotation. For one, it left the writer vulnerable to challenge by readers: letting them know the sources and the basis of your reasoning gave them room to argue another view. This discomfort the author of the *Recherches* was willing to put up with. What bothered him was that fellow historians were taking the documents he had laboriously discovered and published in the first two Books of the *Recherches*—about the origins of parlements and of royal offices, for example—and using them as if they were their own finds. When reproached, they replied that "ancient books belonged to them as much as to me, forgetting . . . that, when gold has been purified from a mine, it is much easier to put to use." He had no ill-will against those who acknowledged his enterprise, so he wrote to his friend Pierre Pithou, but those who were silent about him had committed "true theft" ("un vray larcin"). To prevent further theft, Pasquier withheld the rest of his *Recherches* from the printers for decades, allowing only trusted friends to see the growing manuscript. Meanwhile, he told the world his complaint by publishing his letter to Pithou, along with another one exposing the antics of self-styled "authors" who use the printing press to copy, abbreviate, or patch together the work of others and put their own names on the cover.[14]

And then he changed his mind. In 1596, after thirty years and in a time of new hope that followed the end of the Wars of Religion, Pasquier ordered the printing of a fresh edition of the *Recherches*, many times its former size, to be followed by an even larger one a decade later. He had rethought his sense of property in his historical findings. An old man of seventy-eight, he informed his readers that people had followed in his footsteps and used his material. To those who "had him done the honor to recognize it came from him, he gave it willingly and wanted it to be thought that it belonged to them, as if it were part of their own estate" ("de leur tréfonds"). As for those who, out of "ungrateful ambition, borrowed from him, even transcribing whole phrases word for word without accounting it," he pardoned them, for, no matter how much they took, he had much left for his private delight. Besides, "I am writing only for my France, not for myself." May my gift bring France profit and delight.[15]

In a century when the joys and risks of printing were more and more apparent, Pasquier decided his independent labor had created a text in which he had property rights; he was willing to let others appropriate his work so long as they

[13] Pasquier, *Les Recherches* (1607), Book I, introduction: 1.

[14] Pasquier, *Les Recherches* (1607), Book I, introduction: 2; *Les Lettres d'Estienne Pasquier conseiller et advocat general du Roy en la Chambre des Comptes de Paris* (Paris, 1586), Book VIII, letter 1, 226^{r}–230^{v}; Book X, letter 7, 316^{r}–318^{v}. There were six re-editions of Books I and II of the *Recherches* from 1567 to 1594, the years when Pasquier was circulating the additional books only in manuscript (Thickett, *Bibliographie*, 32, 41).

[15] *Les Recherches de la France, Reveuës et augmentées de quatre Livres* (Paris, 1596); *Recherches* (1607), 2–3. The posthumous edition of 1621, assembled from Pasquier's library by his sons, included three further Books as well as additions to the early ones.

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gave him credit for it but would rather limit its use than let it go to thieves. Once he had redefined his *Recherches* as a labor of delight, done for the higher cause of "our France," he could give it away without too many pangs, even to the ungrateful. SOME 160 YEARS LATER, in 1764, two British historians were politely and formally corresponding about their strong differences over the interpretation of events in seventeenth-century England. By now, David Hume had published the final volumes of his *History of England from the Invasion of Julius Caesar to the Revolution in 1688*, while Catharine Sawbridge Macaulay had just brought out the first volume of her *History of England from the Accession of James I to that of the Brunswick Line* and had sent a copy to the Scottish philosopher-historian with her compliments.[16] He thanked her for "the agreeable present," noted with some irony how often he seemed to be at issue in her narration, and remarked, "I flatter myself that we differ less in facts, than in our interpretation and construction of them." They disagreed, he said, "in some original principles," and went on to state his in a way that could only confirm Macaulay's belief in his Tory partiality: all forms of government, from monarchy to democracy, were "equally legal if established by custom and authority" and that meant there were "obligations to obedience and allegiance" toward the lawful monarchies of Elizabeth, James I, and Charles I. Macaulay was indeed a noble defender of liberty, but the seventeenth-century "partizans of that cause . . . disgraced it by their violence, and also by their cant, hypocrisy and bigotry."[17]

Macaulay's answer, while expressing "all imaginable esteem for so great an ornament to the republic of letters," spelled out their divergence in a way that must have read to Hume like more "senseless clamour" from the Whigs. "Your position that all governments established by custom and authority carry with them obligations to submission and allegiance does, I am afraid, involve all reformers in unavoidable guilt, since opposition to established error must needs be opposition to authority . . . I think the arbitrary princes of the Stuart line took an effectual way to secure themselves from female opposers, since cropping off ears close to the head, slitting of noses, and branding of foreheads must needs be as formidable to women as Caesar's attack on the face was to the Roman petit-maîtres."[18]

Hume and Macaulay were to differ in their views about the English revolutions and the crown until the end of their lives, but in several ways their careers as

[16] David Hume, *The History of England, from the Invasion of Julius Caesar to the Revolution in 1688*, 8 vols. (London: Thomas Cadell, 1770); the first complete edition had appeared in 1762 in six volumes, published by Andrew Millar, Cadell's partner and predecessor. Catharine Macaulay, *The History of England from the Accession of James I to that of the Brunswick Line*, 8 vols. (London: J. Nourse, *et al.*, 1763–83). George Macaulay, husband of Catharine Sawbridge Macaulay, to David Hume, 22 March 1764, asking if he had received his wife's book, in John Hill Burton, ed., *Letters of Eminent Persons addressed to David Hume* (Edinburgh and London, 1849), letter 20, 111–12.

[17] David Hume to Catharine Macaulay, 29 March 1764, in *The European Magazine and London Review*, 4 (November 1783): 331, reprinted in Raymond Klibansky and Ernest C. Mossner, eds., *New Letters of David Hume* (Oxford, 1954), letter 40, 80–82.

[18] Catharine Macaulay to David Hume, undated [April? 1764], in *The European Magazine and London Review*, 4 (November 1783): 331–32.

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historians resemble each other.[19] Neither practiced their historian's art within the university walls: true, the University of Edinburgh had just instituted its first chair in history as Hume began his student years there in 1723, but when he applied for a professorship twenty years later (in philosophy, not history), he was refused for suspicion of "Heresy," "Skepticism," and "Atheism." As for Macaulay, women were excluded from Oxford and Cambridge, which she dismissed in return as mere "seminaries," where "the study of history is little cultivated."[20]

History was still primarily a literary rather than an academic discipline and one which the flourishing business of printing and the expansion of readers' markets could make into a profitable venture. Hume's *History of England*, after poor sales for the first volume, had enormous and enduring success within a decade: as he phrased it, "the copy-money given me by the booksellers much exceeded any thing formerly known in England; I was become not only independent, but opulent."[21] Macaulay's *History of England* did very well from the start, accorded a reception, in the words of one review, "not less flattering to the ambition, than satisfactory to the interest of the writer." Booksellers vied to publish the volumes that she wrote between 1763 and 1771, and, although the response to later volumes was cooler, in the early years of the revolution her *History* went into French, just as Hume's had three decades before.[22]

Both Hume and Macaulay had been touched by the "frenzy of renown" (to use Leo Braudy's apt phrase) in a century in which market sales and literary criticism were effacing the historic role of the patron.[23] The *European Magazine* said in 1783 that Macaulay had "experienced more of the extremes of adulation and obloquy than any one of her own sex in the literary world." Hume lived in the same see-saw climate. For Macaulay, the obloquy was associated perhaps with her radical political tracts but certainly with her second marriage in 1778 to a man twenty-seven years her junior: she was greeted with a charivari of pamphlets and remarks.[24] For Hume, the wrath was especially directed against the alleged

[19] For the life of David Hume, there is the major biography of Ernest Campbell Mossner, *The Life of David Hume*, 2d edn. (Oxford, 1980). For the life of Catharine Macaulay, there is Lucy Martin Donnelly, "The Celebrated Mrs. Macaulay," *William and Mary Quarterly*, 3d ser., 6 (1949): 173–207. Carla Hay, Bridget Hill, and Barbara Schnorrenberg have research on her underway and publications planned.

[20] Mossner, *Hume*, 39, 44–46, 153–62. Macaulay, *History of England from the Accession*, 1: xiv.

[21] Mossner, *Hume*, 311–16. David Hume, *The Life of David Hume, Esq. Written by Himself* (London: W. Strahan and T. Cadell, 1777), 24–25.

[22] *European Magazine*, 4 (1783): 332–33; Donnelly, "Celebrated Mrs. Macaulay," 182. There are both quarto and octavo editions of Macaulay's *History*, and she had publishers in Dublin as well as London. Cadell, one of Macaulay's publishers, also had investment in Hume's *History*. *Histoire d'Angleterre, depuis l'avènement de Jacques I jusqu'à la révolution, par Catherine [sic] Macaulay Graham, traduite en français et . . . enrichie de notes par Mirabeau*, 5 vols. (Paris, 1791–92).

[23] On this whole process, see Braudy's important work, *The Frenzy of Renown*, especially 361–80.

[24] *European Magazine*, 4 (1783): 334; Donnelly, "Celebrated Mrs. Macaulay," 187–88. Among her tracts are those mentioned in n. 39 below and *Loose Remarks on Certain Positions to be found in Mr. Hobbes's Philosophical Rudiments of Government and Society, With a Short Sketch of a Democratical Form of Government, in a Letter to Signior Paoli* (London, 1767); and *Observations on a Pamphlet entitled, Thoughts on the Cause of the Present Discontents* (London, 1770). On these pamphlets, see Barbara B. Schnorrenberg, "The Brood Hen of Faction: Mrs. Macaulay and Radical Politics, 1765–1775," *Albion*, 11 (1979): 33–45. Pamphlets mocking her were *A Bridal Ode on the Marriage of Catherine and Petruchio* (London, 1779) and *A Remarkable Moving Letter* (London, 1779).

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godlessness of his philosophical writings and was spread over decades; at his very end, zealots were claiming he lied when he reported tranquillity in the face of death.[25] The question is how, in the theater of history publication, with its own back and forth between fame, obscurity, and abuse, Hume and Macaulay constructed their relation to Clio.

Hume approached history with skepticism: "The study of history confirms the reasoning of true philosophy."[26] Past events, known with at least middle-level certainty, were needed as evidence for political and moral principles. And what an opportunity history offered for developing critical methods! One could take, for example, the opinion of a Mr. Carte that Perkin Warbeck was a true Plantaganet rather than a royal impostor in the late fifteenth century and refute it by rigorously evaluated testimony.[27] Of course, Hume ultimately had to admit that his own prior views influenced his interpretation of the evidence, if only as he changed his mind and altered what he had said in early editions. He had initially prided himself on the impartiality of his *History*, uninfluenced by "present power, interest, authority, and the cry of popular prejudices," but, rereading his historical authors, he realized that his criticism of James I and Charles I for levying taxes without consent of Parliament smacked of "Whig Rancour." He went through his text making, so he said, "above a hundred alterations . . . all of them invariably to the Tory side." At least he did this while the Whigs were in power, so he could not be accused of seeking patronage or place.[28]

Macaulay approached history with belief, religious and especially political—indeed, Caroline Robbins has shown her to be an important figure in republican circles in the reign of George III.[29] A righteous God had planned a world of

[25] Mossner, *Hume*, 597–99, 621–22. An example of the attack is [George Horne], *A Letter to Adam Smith, LL.D. on the Life, Death, and Philosophy of his Friend David Hume, Esq. By one of the People Called Christians* (Oxford, 1777), with further editions published by the Society for Promoting Christian Knowledge as late as 1799.

[26] On Hume as a historian, see Mossner, *Hume*, 301–11; David Fate Norton, "History and Philosophy in Hume's Thought," in David Fate Norton and Richard H. Popkin, eds., *David Hume: Philosophical Historian* (Indianapolis, 1965), xxxii–l. The latter work includes a bibliography of works on Hume as historian, liii–lv, to which can be added Leo Braudy, *Narrative Form in History and Fiction: Hume, Fielding, and Gibbon* (Princeton, N.J., 1970); and Richard H. Popkin, "Hume: Philosophical versus Prophetic Historian," in Kenneth R. Merrill and Robert W. Shahan, eds., *David Hume, Many-sided Genius* (Norman, Okla., 1976), 83–96.

[27] Hume, *History of England* (1770), 3: 479–83. See also Hume's letter to the Reverend John Douglas, in which he assesses the likelihood that a French subsidy to Charles II could have occurred in 1661–62 (David Hume, *The Letters of David Hume*, ed. J. Y. T. Grieg, 2 vols. [Oxford, 1932], 2: 226–27).

[28] Hume to Gilbert Elliot, 12 March 1763, in Klibansky and Mossner, *New Letters*, 69–71, reprinted in Norton and Popkin, *Hume*, 406–07. Hume, *Life*, 18–19, 22–23. For modern assessment of Hume's *History* as more impartial than this Tory outburst would imply, see Mossner, *Hume*, 310–11; and Braudy, *Narrative Form*, 36–37.

[29] On Macaulay's historical work, see Bridget Hill and Christopher Hill, "Catharine Macaulay and the Seventeenth Century," *Welsh History Review*, 3 (1967): 173–207; Lynne E. Withey, "Catharine Macaulay and the Uses of History: Ancient Rights, Perfectionism, and Propaganda," *Journal of British Studies* (Fall 1976): 59–83; and N. Z. Davis, "Gender and Genre: Women as Historical Writers, 1400–1820," in Patricia Labalme, ed., *Beyond Their Sex: Learned Women of the European Past* (New York, 1980), especially 167–72. Further material on her political circle of radical Whigs and her role as pamphleteer can be found in Caroline Robbins, *The Eighteenth-Century Commonwealthman* (Cambridge, Mass., 1959), 356–77; and Schnorrenberg, "The Brood Hen of Faction." On the politics of the 1760s, see the important study of John Brewer, *Party Ideology and Popular Politics at the Accession of George III* (Cambridge, 1976).

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ultimate human perfectability through the use of reason. Macaulay took up her pen to defend "the cause of liberty" against those who, out of neglect, party prejudice, or selfish ambition, had falsified the past and insulted the memory of the illustrious resisters of Stuart tyranny.[30] The historian served liberty by digesting "voluminous collections" and giving the public a "true and accurate report of their sense." It was in gathering sources, rather than in a critical assessment of authors, that Macaulay saw her rightful labor: she intended to provide a "just information of facts," "uncontrovertible argument, founded on fact." Her footnotes contain references to manuscripts and tracts in the British Museum, to which Hume, reading mostly in the Advocates' Library in Edinburgh, came late, if at all.[31]

Although Macaulay did not share Hume's worry about deciding what a fact was, she did care as he did about being "impartial" and "disinterested," that is, not yielding to party spirit in interpreting events and characters. Her treatment of Charles I was, she thought, a case in point. She had made convincing and justifiable the reasons of those who put him to death yet had tried "to do justice to that part of his conduct which [she] thought truly great." And then, echoing a phrase of Hume, she wrote, "I shed many tears whilst I was writing his catastrophe."[32]

Skeptic and believer—how did Hume and Macaulay respond to other historians, to rivals, or to those with whom they disagreed? Hume's reactions were a combination of sarcasm, jealousy, and good sportsmanship—ultimately allowing a relatively unimpeded flow of history books as his friend Adam Smith wanted a relatively unimpeded flow of commodities.[33] Hume characterized John Dalrymple's *Memoirs of Great Britain* in letters to his publisher as "ranting, bouncing," full of "Antitheses and Rant and Whiggery"; since the publisher had paid Dalrymple so much for the *Memoirs*, he would have to pay "the equivalent of a parliamentary Subsidy" to get another history volume from him, Hume.[34] As for William

[30] Review of Macaulay's *A Treatise on the Immutability of Moral Truth* in *The European Magazine and London Review*, 4 (July 1783): 37–39; Hume's skepticism is one of Macaulay's targets; Macaulay, *History of England*, 1: vii–xviii, introduction; 6: v–xiv, preface dated January 1781.

[31] Macaulay, *History of England*, 1: x, 2; 6: vii. Robbins, *Commonwealthman*, 15, 267. Macaulay was less sanguine about the public judging "facts" aright when she published her sixth volume than when she published her first: in 1763, she thought that individuals might err in assessing facts but "the public judgment is infallible" (1: x); in 1781, she realized how hard it was for "uncontrovertible argument founded on fact . . . to influence the minds of a nation in favour of a democratic form of government, who from the beginning of time have been under the rule of regal sway" (6: vii). On Hume's connection with the Advocates' Library and his consultation of manuscripts in the British Museum, see Mossner, *Hume*, 249–55, 316, 395, 401. On Hume's attitude toward "'research' scholarship" ("I have inserted no original Papers, and enter'd into no Detail of minute, uninteresting Facts") and his justification for "rewriting" English history when so many historical "monuments" already existed, see *ibid.*, 316 and Hume's letter to Horace Walpole in Grieg, *Letters*, 1: 284–85, letter 152.

[32] Macaulay, *History of England*, 6: i, xii–xiii. Hume, *Life*, 17–19, on the reaction to the first volume of his *History*: "I was assailed by one cry of reproach . . . Whig and Tory . . . patriot and courtier, united against the man who had presumed to shed a generous tear for the fate of Charles I and the earl of Strafford."

[33] Hume's ideas on free commercial exchange are found in his *Political Discourses* of 1752 ("Of Commerce"). For his reaction to the publication of *The Wealth of Nations*, see his letter in 1776 to Adam Smith in *Letters*, 2: 311, letter 517; and Mossner, *Hume*, 270–71.

[34] Hume, *History*, 2: 529–31. Hume to William Strahan, 11 March and 25 March 1771, in *Letters*, 2: 238, 242, letters 454–55. When the second volume of Dalrymple's *Memoirs of Great Britain* was published in 1773, Hume wrote the publisher "that it gives me great Satisfaction to find that there is not one single

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Robertson's *History of Scotland*, Hume found the work of "uncommon Merit" and helped its author demand a high price from the publisher, to whom he also wrote, "it will be an Amusement to the Reader to compare our Method of treating the same Subject." He praised the book wherever he went, dealing with his envy when Robertson's *History* did much better than his own latest volume by defining him as a friend and protégé and by periodic teasing: I saw a pound of raisins wrapped up by a grocer in "a leaf of your History," he wrote Robertson, or "Here I sit near the historical summit of Parnassus . . . and you have the impudence to squeeze yourself by me."[35]

And what of Hume on Macaulay? He was better prepared than most to have a woman for a serious rival. Had he not said in an early essay that "there is nothing which I would recommend more earnestly to my female readers than the study of history"? Had he not already found the bluestocking Elizabeth Montagu "a lady of great distinction"?[36] Thus, when Macaulay sent him her first volume, he could close his letter to her in 1764 "expressing my great esteem of your history" and then could find himself troubled by her success. As one observer wrote: "Nothing ever gave Hume more real Vexation than the Strictures made upon his History in the House of Lords by the great Lord Chatham [William Pitt]. They were indeed carried to an extraordinary length when Mrs. McCauly, as an Historian was preferred to him, and her constitutional Writings, were declared to be the only Antidote to his Poison."[37] Hume published no answer to her writings, for he had long before determined never to answer anyone in print. Instead, he wondered to his publisher about the high sales of her *History* despite its "Whiggery"; he commented with some satisfaction to Robertson that "the Sanhedrim at Mrs. Macaulay's condemns you as little less a friend to government and monarchy than myself"; he noted, not long before his death in 1776, that "her Muse now seems to be mute."[38]

Mistake in my History, either great or small, which it gives me occasion to correct." He has, however, something to correct in Dalrymple's work (Hume to Strahan, 20 March 1773, in *Letters*, 2: 278, letter 489).

[35] Mossner, *Hume*, 396–98. Hume to Andrew Millar, 6 April 1758 in *Letters*, 1: 273, letter 145; Hume to William Robertson, March? 1759 and 12 March 1759, in *Letters*, 1: 300–02, letters 163–64.

[36] Hume, "Of the Study of History," published in 1741 in *Essays Moral and Political* and withdrawn by Hume from editions of the *Essays* after 1760 as "frivolous"; reprinted in Norton and Popkin, *Hume*, 35–39. Hume to Rev. Hugh Blair, 6 October 1763, in *Letters*, 1: 404, letter 217; Mossner, *Hume*, 395. Hume's attitude toward Macaulay was surely more respectful of her qualities than that of his friend, the painter Alan Ramsay, who sent Hume early news of Macaulay's first volume: "Somebody under the name of Mrs. Catharine Maccauley has written a romance, called 'James the First,' the secret design of which is to abuse you and me, and all the other people of consequence, whom she calls the creatures of a court, and the tools of tyranny. I am meditating revenge . . . What if we should bring about a match betwixt this woman (if there be such a woman) and Patriot Wilkes? Were it not a *consummation devoutly to be wished*? I think the patriotic invectives, the kicks and the cuffs, that the spirit of liberty would produce *de part et d'autre*, would not only revenge us, but would give the whole parish more true insight into politics than all your essays put together" (Alan Ramsay to David Hume, 8 November 1763, in Burton, *Letters of Eminent Persons*, 29–30).

[37] *European Magazine*, 4 (November 1783): 331, reprinted in *New Letters*, 82, letter 40. Lord Charlemont's "Anecdotes of Hume," reprinted in Mossner, *Hume*, 310.

[38] Hume, *Life*, 15. Hume to Strahan, 25 March 1771; Hume to Robertson, 28 March 1769; Hume to Blair, 13 May 1776, in *Letters*, 2: 242, letter 455; 199, letter 428; 321, letter 524. In the letter to Blair,

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In fact, Macaulay's Muse was not silenced—she had recently published two political tracts—and before long, new volumes of her *History* appeared, in which she again played herself off against Hume.[39] Macaulay's public reactions to him and to other historians with whom she disagreed were efforts at transcendence, that is, efforts to stand above private rivalry and speak only of history's higher goals. (Indeed, the private was a dangerous arena for her: she knew what happened "when personal invective supplies the place of argument, and the reputation of authors are attacked in order to decry their works."[40]) In a preface published in 1781, with Hume in his grave for several years, she gave her assessment. Hume was a man of "genius and profound sagacity," who could have stood "at the head of all our historians." But, either because he feared the enemies he would rouse if he wrote an impartial history or because, as she thought more likely, of the "prejudices he had entertained," his history, "whilst it serves as an elegant pastime for the hours of leisure or idleness, leaves the reader perfectly ignorant as to characters, motives and often facts." His work was enjoying "an unrivalled popularity," with dire consequences for the principles of the revolution and the public cause.[41] Yet was this really her final image of Hume? How else did the polite exchange of 1764, memorializing a mutual esteem between her and the philosopher-historian, get published in a 1783 journal if not by her hand?

When it came to their own fame and renown, Hume and Macaulay pictured themselves and their presence in their historical writings rather differently. Hume was very frank about his desire for renown, never felt it was compromised by his simultaneous desire for income from his publications, and relativized both appetites by light self-mockery. He could joke with Robertson about being crowded at Parnassus and insist that the fight between their history books would make less brouhaha than a current boxing match. When beseeched by his publisher to do a final volume for his *History* ("it is the only thing wanting to fill up the Measure of your Glory as the Great Historian and Philosopher of the Eighteenth Century"), he could decline, "I'm too old, too fat, too lazy, and too rich."[42] Similarly, he resisted for some time his publisher's urging that the *History* include his portrait: "a superfluous expense," he thought, the money better spent on print and paper.

Hume went on to report with light sarcasm, the gifts and fortune bestowed on Macaulay by the Reverend Thomas Wilson, "a man zealous for Liberty."

[39] Catharine Macaulay, *A Modest Plea for the Property of Copyright* (London, 1774); *An Address to the People of England, Scotland and Ireland, on the Present Important Crisis of Affairs* (London, 1775); *History of England from the Revolution to the Present Time in a Series of Letters to a Friend* (London: E. and C. Dilly, T. Cadell, J. Walter, 1778). Vols. 6 and 7 of *The History of England from the Accession of James I* appeared in 1781, vol. 8 in 1783.

[40] Macaulay, *History of England from the Accession*, 6: xiv. She goes on, in what is surely a reference to the anonymous pamphlets in 1779 ridiculing her for her second marriage, to talk of rising above difficult situations: "In this case, an individual . . . must look down with contempt on the angry crowd, nor suffer their fierce and loud clamours, in any respect, to divert him from pursuing the grand object of his honest ambition."

[41] Macaulay, *History of England from the Accession*, 6: vi. In her next volume, she attacked Hume strongly for his "ridiculous charge of corruption" against Algernon Sidney: Hume's "partiality on the side of the court in this part of his history is a greater disgrace to his admirable genius and profound sagacity than any other page of his historical writings"; *History of England from the Accession*, 7: 494.

[42] Hume to Robertson, March? 1759, in *Letters*, 1: 300–01, letter 163; Mossner, *Hume*, 555–56.

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ILLUSTRATION 2: Portrait of David Hume, painted by Donaldson and engraved by Ravenet. Frontispiece to Hume's *History of England* (London, 1770). Courtesy of Rare Books and Special Collections, Princeton University Library.

When the portrait finally appeared in the 1770 edition—eighteen years after the first volume—it had few traditional symbols of renown but just two books, marked History and Philosophy, and two quills (Illustration 2).[43]

Not long before Hume died, he recorded his sentiments about reputation in a

[43] Hume to Millar, 17 May 1762 and 19 October 1767, in *Letters*, 2: 359, letter 193; 2: 169–70, letter 411.

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ILLUSTRATION 3: Portrait of David Hume, engraved by Collyer after Hume's death with added laurel and personifications of Philosophy and History. Used for subsequent editions of the *History*. From David Hume, *History of England* (London, 1802). Courtesy of Princeton University Library.

brief autobiographical text—*My Own Life*—which he wanted appended to all future editions of his works.[44] A self-portrait worthy of the author of the *Treatise of Human Nature*, it presents Hume as seized early on "with a passion for literature," which shifts to "my love of literary fame, my ruling passion." But the narrative is mostly made up of disappointments and mortifications: books "deadborn from the

[44] Hume to Adam Smith, 3 May 1776, and Hume to Strahan, 8 June 1776, in *Letters*, 2: 318, letter 522; 2: 323, letter 525.

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ILLUSTRATION 4: Portrait of Catharine Macaulay as Libertas, designed in 1767 by I. B. Cipriani, engraved by I. Basire, and used as frontispiece to Volume 3 of the London, 1767, edition of the *History of England.* Courtesy of Rare Books and Special Collections, Princeton University Library.

press," fallen, ignored. The first volume of the *History* "sinks into oblivion" after an initial outcry of indignation. The second does better, but the third is greeted with hostile "clamour," the reign of Elizabeth being found "particularly obnoxious." Hume refuses to be discouraged and the story moderates into some success, as he finds he is making money from his books. He ends asking the reader to decide if his vanity is misplaced.[45] *My Own Life* is one of Hume's most characteristic performances: his posthumous reputation is assured each time a reader picks up an edition (and his publisher added laurel to the portrait, Illustration 3), but that edition will always carry with it his ironic tale about the love of Fame.

[45] *My Own Life* in *The History of England, from the Invasion of Julius Caesar to the Revolution of 1688* (London: T. Cadell, 1792), v–xv. Also reprinted in Mossner, *Hume*, Appendix A.

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Catharine Macaulay presented her desire for reputation as undiluted by hope for material gain and as wholly given over to the cause of liberty. Her first volume included verses taken from the Scotsman James Thomson, in which the poet says his Muse must serve the Goddess of Liberty. Should she sell her work (Thomson had written "song," Macaulay changed it to "WORK") to Liberty's foes, may it sink into oblivion.[46] When Macaulay's volume is then given "favorable reception," she tells readers she is grateful on behalf of the "friends of Liberty." That she drove a hard bargain with her publishers and lived with some extravagance led one of those radical friends—the eminent Thomas Hollis—to comment privately, "It would be a sad case to write of Liberty . . . at a price." But Hollis respected her enough to help her plan a frontispiece for her third volume in 1767, associating her renown with republicanism (see Illustration 4). She is depicted in elegant Roman profile as Libertas, a replica of an ancient Roman coin in honor of the revolutionary Lucius Junius Brutus, and framed by a Roman victor's wreath of oak leaves. Underneath her portrait is the reverse of the coin, showing Brutus's own sons being led off to execution because they conspired with the deposed king, Tarquin.[47]

A dozen years later, another portrait of Macaulay appeared, more daring in its representation of her relation to her subject (see Illustration 5). The frontispiece to her volume about the eighteenth century, *The History of England from the Revolution to the Present Time in a Series of Letters to a Friend*, it depicts her standing erect against a classical landscape. Next to her, a stone is engraved with worthy sentiments: "Government a power delegated for the happiness of mankind, conducted by wisdom, justice, and mercy." Her hand holds her quill; her elbow rests on the five existing volumes of her *History of England*.[48] She is Clio, her own

[46] Macaulay, *History of England from the Accession*, 1: xix. *The Works of James Thomson With his Last Corrections and Improvements* (London, 1762), 249–50. Macaulay's *Modest Plea for the Property of Copyright* (Bath and London, 1774) argues for a decent reward for "literary labours" and the possibility of providing for one's posterity, but she also defends copyright because it allows writers to be free of dependence and a venal pen.

[47] Macaulay, *History of England from the Accession*, 2: Advertissement, 10 Jan 1765. On Thomas Hollis's comments about and aid to Macaulay, see Donnelly, "Celebrated Mrs. Macaulay," 182; he was an important republican and defender of liberty in the American colonies (Robbins, *Commonwealthman*, chaps. 7–8). The portrait, dated 1767, was designed by I. B. Cipriani and engraved by I. Basire for the printing in London, 1767, of the third volume of the *History*. For the original Roman coin on which the picture of Macaulay is based, see Harry Thurston Peck, ed., *Harper's Dictionary of Classical Literature and Antiquities* (New York, 1965), 225. By royalist custom, oak wreaths were worn on 29 May which was the birthday of Charles II; Macaulay in contrast was giving the wreath its Roman meaning (Shakespeare, *Coriolanus*, I, iii, 12) and also associating it with the patriotism of the British oak. Her youthful reading had been in the annals of the Greek and Roman republics "that exhibit Liberty in its most exalted state" (*History*, 1: vii). Since she believed that it was "contrary to the duty of an historian to spare even the memory of a parent, if he was found defective in those patriotic virtues which eminently affect the welfare of society" (6: xiii), she may have found Brutus's refusal to spare his children understandable.

[48] Macaulay holds in her hand a paper on which is the name Dr. Wilson of Walbrook, that is, the Reverend Thomas Wilson, absentee rector of St. Stephen's Church in Walbrook. Patriot and enthusiast for liberty, the elderly Wilson lived in Bath, where he bestowed house and moneys on the widowed Macaulay and her daughter and commissioned a statue of Macaulay with a history book. She defined him as a "friend" rather than as a patron, and it is to him that the historical letters of the 1778 *History* are addressed. On the literary inventiveness of this epistolary history, see Davis, "Gender and Genre," 169. On the character of her relationship with Wilson and its termination when Macaulay married her young second husband, see *European Magazine*, 4 (November 1783): 329–30; and Donnelly, "Celebrated Mrs. Macaulay," 184–87.

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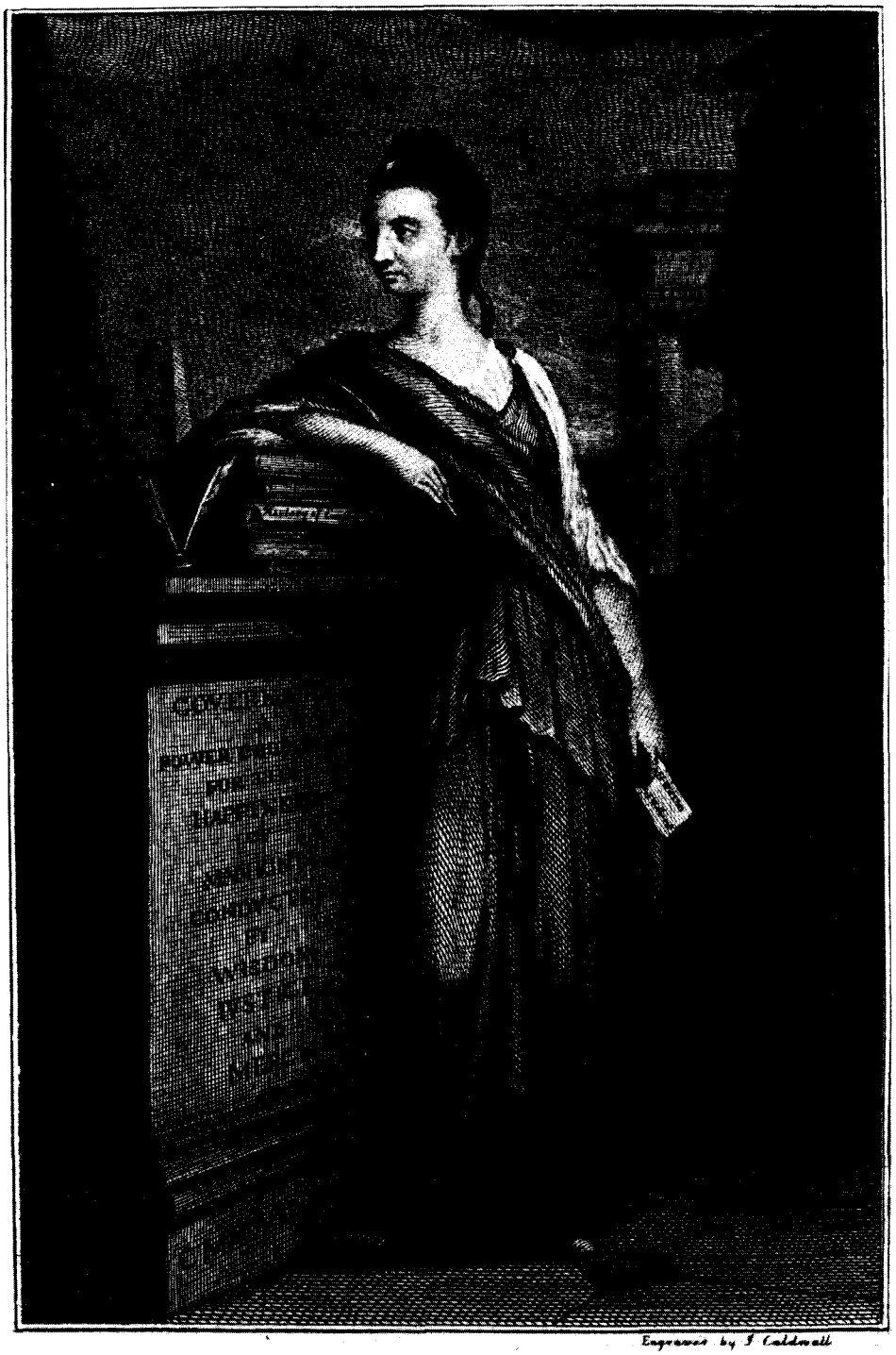


ILLUSTRATION 5: Portrait of Catharine Macaulay as Liberty and Clio, frontispiece to her *History of England from the Revolution to the Present* (Bath and London, 1778). Courtesy of Thomas Fisher Rare Book Library, University of Toronto.

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muse; she is Liberty; she is Catharine Macaulay. These elisions, which have important precedents in self-representation by artistic and literary women,[49] have a double potentiality. On the one hand, they can obscure the tension between the living historian and the eternal body of history, making her prophetic or indignant rather than self-correcting. On the other, they can internalize that tension in a creative way, reminding the historian of a transcendent task and personal responsibility. Catharine Macaulay practiced sometimes one mode, sometimes the other.

On the stage of eighteenth-century historical production, Hume and Macaulay each helped relieve "the frenzy of renown," Hume by showing its roots in the passions and laughing at its vicissitudes, Macaulay by insisting it be linked to a high goal beyond the self.

LET US MOVE FORWARD ONCE AGAIN 160 years. We are in the familiar landscape of history as a university subject, of history as a profession, of history journals, learned societies, and the rest. We are going to pause in the 1930s, when Marc Bloch was publishing reviews of the work of Eileen Power in the journal he edited with Lucien Febvre, the *Annales d'histoire économique et sociale*. As always with his reviews, Bloch used them to talk about the general shape of historical endeavor and where it might go. In 1933, a new volume of the *Cambridge Medieval History* was at issue, along with other recent works of a synthetic nature. Bloch remarked that the *Annales*, with its stress on comparative history, was not going to complain about "a taste for large horizons," but he hoped that good national histories, which were essential for comparison, would not be lost sight of along the way. Eileen Power's chapter, "Peasant Life and Rural Conditions (c. 1100 to c. 1500)," he found "solid and elegantly presented," but, "constructed rather like a great painting, it perhaps does not do sufficient justice either to the great transformations common to all of the West or to regional contrasts."[50]

In 1934, it was the turn of *Studies in English Trade in the Fifteenth Century*, edited by Power and Michael Postan. Here Bloch had no reservations. He liked the fact that the volume emerged from the collective endeavor of a London seminar and wished that the organization of higher education in France allowed scholars to work in similar groupings. He liked the way the customs accounts had been put to use to give a precise and wholly new picture of the character of the exchange economy. He thought Power's essay on "The Wool Trade" and Sylvia Thrupp's

[49] On this topic, see the article by Mary D. Garrard, "Artemisia Gentileschi's Self-Portrait as the Allegory of Painting," *Art Bulletin*, 62 (March 1980): 97–112; and Judith E. Stein, "The Iconography of Sappho, 1775–1875" (Ph.D. dissertation, University of Pennsylvania, 1981), chap. 4. The portrait of Macaulay as Clio and Liberty has a significant place in this tradition. It was followed the next year by Richard Samuel's painting of *The Nine Living Muses of Great Britain*, nine artists and literary women in the temple of Apollo; Catharine Macaulay is there as Clio (National Portrait Gallery, *Complete Illustrated Catalogue*, comp. K. K. Yung [New York, 1980], 668, no. 4905).

[50] Marc Bloch, "Manuels ou synthèses?" *Annales d'histoire économique et sociale*, 5 (1933): 67–71. Eileen Power, "Peasant Life and Rural Conditions (c. 1100–c. 1500)," in J. R. Tanner, *et al.*, eds., *The Cambridge Medieval History*, 8 vols. (New York and Cambridge, 1924–36), 7: 716–50.

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on "The Grocers of London" provided a remarkable portrait of the local structure of economic life at the end of the Middle Ages.[51]

From the other side of the Channel came a similar response to Bloch. Power was on the editorial board of the *Economic History Review*, in whose pages R. H. Tawney marveled at Bloch's new book, *Les Caractères originaux de l'histoire rurale française*, at its combination of "mastery of specialist research with the ability . . . to elucidate the significant problems of agrarian history." By 1936, Bloch was collaborating with the *Review*, sending over bibliographies on the economic history of France, and, not long after that, Power helped arrange his Cambridge lectures on feudalism and won Bloch's cooperation for the new *Cambridge Economic History of Europe*. When the first volume appeared in 1941, Eileen Power was dead, snatched away the year before at fifty-one, but, despite the war, Bloch's chapter titled "The Rise of Dependent Cultivation and Seigniorial Institutions" had reached her hands in time, as had an essay on English villages by American historian Nellie Neilson. When the second volume came out after the war, Bloch, of course, was also gone, but, as Postan said, "His last letter to the editors, sent through clandestine channels a few months before he was shot by the Gestapo, contained enquiries and suggestions about [the project]."[52]

It is these two figures, Marc Bloch and Eileen Power, who make up my last pair, to be looked at as they moved through their careers and staked their positions within their discipline. Since both were innovators and reformers, the issue of historical succession is a central one: how did they relate to their teachers, against whom they may have rebelled? whom did they define as co-reformers? what kind of heirs did they expect?

Descendant of an old Jewish family from Alsace and Lorraine, son of a professor of Roman history at Lyon and Paris, Marc Bloch was no newcomer to the university world. He studied at the Ecole Normale Supérieure and heard Charles-Victor Langlois and Charles Seignobos, those eminent figures, lecture on history as a science of documentary criticism and classification of social facts; he studied at Leipzig and heard Karl Bücher talk about the distinctive characteristics of primitive economies.[53] After this, it was research in Paris for his thesis, lycée teaching, war service, and then Bloch was given a post at the Faculty of Arts at Strasbourg, where he forged the friendship with Lucien Febvre that was given expression in 1929 in their new journal, the *Annales d'histoire économique et sociale*.

[51] Marc Bloch, "Etudes sur le commerce anglais au XV^{e} siècle," *Annales d'histoire économique et sociale*, 6 (1934): 316–18. Eileen Power and M. M. Postan, eds., *Studies in English Trade in the Fifteenth Century* (London, 1933).

[52] R. H. Tawney, review of Marc Bloch, *Les Caractères originaux de l'histoire rurale française* in *Economic History Review*, 4 (1932–34): 230–33. Bloch has a bibliography, drawn up with Paul Leuilliot, in *Economic History Review*, 7 (1936–37). *The Cambridge Economic History of Europe*, 7 vols. (Cambridge, 1941–78), 1: *The Agrarian Life of the Middle Ages*, eds. J. H. Clapham and Eileen Power, preface, chaps. 6–7; 2: *Trade and Industry in the Middle Ages*, eds. M. M. Postan and E. E. Rich, preface.

[53] It is worth looking at the manuals of Langlois and Seignobos to see what their teaching actually was: Charles-V. Langlois and Charles Seignobos, *Introduction aux études historiques* (Paris, 1898); and Charles Seignobos, *La Méthode historique appliquée aux sciences sociales* (Paris, 1901). See also the fine pages of William R. Keylor, *Academy and Community: The Foundation of the French Historical Profession* (Cambridge, Mass., 1975), chap. 4. Bloch on Karl Bücher in *Annales d'histoire économique et sociale*, 4 (1932): 65–66.

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Rejected in his candidacy at the Collège de France—due to anti-Semitism, so Bloch always believed—he was in 1936 named professor of economic history at the Sorbonne, the institution from which his father had retired years before.[54]

Eileen Power was the first in her family to be part of the university world.[55] Her mother died when she was young, her father was cut off from the family much of the time, and she was the responsible oldest of three sisters. Educated at Girton College Cambridge during the years when new stress was being placed on advanced research, she received a first class in her historical examinations in 1910 and went off to Paris for a year at the Sorbonne and the Ecole des Chartes, including study with Charles-Victor Langlois. After two years on a research scholarship at the London School of Economics, she was back at Girton as director of historical studies from 1913 to 1920. Her mentor at Cambridge during those years was George Gordon Coulton of St. John's College, that energetic gatherer of data about medieval life, religion, and customs, who thought "clearer facts" and the denunciation of error the historian's main charge. It was for his series, the Cambridge Studies in Medieval Life and Thought, that she prepared her great economic and social monograph, *Medieval English Nunneries*.[56]

Her manuscript on the nuns finished, she had an adventure that had as much impact on her thought as Bloch's experience in World War I had on his, and more impact on her career. She spent a year in 1920–1921 on a traveling fellowship in India, Burma, China, and Java. In India, she had a shock of recognition: here was the medieval society in all its complexity that she had been studying for the past twelve years. In Java, she began to see how a dance or puppet show could compress within it a whole cultural style and how old and new historical forms lived in tension. China, to which her heart was "irrevocably given," was striking for its imperviousness to economic change (although its farmers, she said, were "the most

[54] Biographical material on Marc Bloch can be found in Charles-Edmond Perrin, "L'Oeuvre historique de Marc Bloch," *Revue historique*, 199 (1948): 161–88; Carole Fink, "Introduction" in Marc Bloch, *Memoirs of War, 1914–15*, Carole Fink, trans. (Ithaca, N.Y., 1980), 15–73; Eugen Weber, "About Marc Bloch," *American Scholar*, 51 (Winter 1981–82): 73–82; André Burguière, "Marc Bloch, 1886–1944," in his edn. of *Dictionnaire des sciences historiques* (Paris, 1986), 88–91; Etienne Bloch, *Marc Bloch: Father, Patriot, and Teacher* (Poughkeepsie, N.Y., 1987). Carole Fink has a full-scale biography forthcoming entitled *Marc Bloch: Historian, Soldier, Patriot*. I am grateful to her for clarifying for me several points about Bloch's life and attitudes. Bloch's suspicions that anti-Semitism and Jewish quotas were behind his rejection at the Collège de France are given in 1936 letters to Febvre, excerpted in *Annales d'histoire sociale*, 7, 1 (1945): 29–30. Weber gives interesting detail on the several factors that played into Bloch's rejection by the Collège.

[55] On Eileen Power's life, one can find information in the obituary by R. H. Tawney in *Economic History Review*, 10 (1940): 92–94 and in Tawney's article on her in L. G. Wickham Legg, ed., *The Dictionary of National Biography, 1931–1940* (hereafter, *DNB, 1931–40*) (London, 1949), 718–19. I have also learned much from discussions with Dr. Sylvia Thrupp, who knew her from the end of the 1920s until her death.

[56] On Girton during Power's day, see Barbara Stephen, *Girton College, 1869–1932* (Cambridge, 1933); B. Megson and J. Lindsay, *Girton College, 1869–1959: An Informal History* (Cambridge, 1961); and M. C. Bradbrook, *"That Infidel Place": A Short History of Girton College, 1869–1969* (London, 1969), 66–67. On George Gordon Coulton, see his *Four Score Years: An Autobiography* (Cambridge, 1943), especially chaps. 32–34; and Gerald Christianson, "G. G. Coulton: The Medieval Historian as Controversialist," *Catholic Historical Review*, 57 (1971): 421–41. His general preface to his series promised yearly errata sheets responding to all queries about "errors of fact" sent in by readers or reviewers; Eileen Power, *Medieval English Nunneries, c. 1275–1535* (Cambridge, 1922), v–vi.

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skilled in the world") and the rationality of its intellectual life.[57]

She returned with a deepened sense of what civilizations are and how to describe them, and with a commitment to comparison as a historical tool. She resigned her post at the women's college, accepted a lectureship at the London School of Economics, an institution with both men and women on its staff and as its students since its beginnings, and became a close collaborator of one of the School's new readers, R. H. Tawney.[58] In the next two decades, as lecturer and then professor of history at the School; as leader with Postan of a celebrated economic history seminar at the Institute of Historical Research in London; as secretary of the new Economic History Society; as author, reviewer, and editor; and as Ford's lecturer in English history at Oxford ("the only woman so far to hold that position," Tawney reminded people), Eileen Power was one of the creators of a new kind of social and economic history in England.[59]

The historical practice that Bloch and Power wished to inspire was in many ways alike, in a few ways different.[60] To begin with, they had similar targets to attack: both were critical of a rural history that confined itself to establishing authentic charters and grants, describing the legal relations between lord and serf, and adding a bit of local color, rather than moving on to agriculture, estate management, rural trade, social domination, and peasant communities. Both thought history must be comparative if it was to have any hope for establishing causes, repeatable connections, and modifying conditions. For Bloch, comparison undermined our assumptions about what was "natural," for Power, about what was socially "habitual." Both thought the past and the present should be in constant exchange, generating questions for historians and perspectives, if not judgments, on the contemporary world. Both thought history needed strong interdisciplinary connections to offer new source material, or relevant factors (like climate and soil), or modes of interpretation with which historians were not familiar. Power's links were especially with the economists and sociologists at the London School of

[57] Eileen Power, *Report to the Trustees of the Albert Kahn Travelling Fellowships* (September 1920 to September 1921). The microfilm of this printed report in the University of Toronto Library (Microfilm no. 426) includes two letters from Power to Coulton: the first, written from Girton College, tells him she has been awarded a Kahn fellowship, thanks him for his "testimonial," and says she is going "to slave at the nuns" to get them to press before she leaves.

[58] Lord Beveridge has written a somewhat chatty history of the School during Power's years there, *The London School of Economics and Its Problems, 1919–1937* (London, 1960). Power was not the first Girton graduate to join its faculty: Lilian Tomm Knowles had had a post since 1904. A major collaborative project of Tawney and Power was *Tudor Economic Documents*, 3 vols. (London, 1924); Power is thanked in the introduction to the first edition (1926) of Tawney's *Religion and the Rise of Capitalism* for having read the manuscript. On the importance of their relationship to Tawney, see Ross Terrill, *R. H. Tawney and His Times: Socialism as Fellowship* (Cambridge, Mass., 1973), 66–67, 69, 82, 107.

[59] Tawney in the *DNB, 1931–40*, 718. On the varieties of historical work being done by women in Power's day, see the excellent article by Bonnie G. Smith, "The Contribution of Women to Modern Historiography in Great Britain, France, and the United States, 1750–1940," *AHR*, 89 (1984): 709–32.

[60] On Bloch's views on history, see the studies mentioned in note 54 above and also Bryce Lyon, foreward to Marc Bloch, *French Rural History: An Essay on Its Basic Characteristics*, Janet Sondheimer, trans. (Berkeley, Calif., 1966), ix–xv; Carlo Ginzburg, preface to Marc Bloch, *I re taumaturghi* (Turin, 1973), xi–xix; and André Burguière, "La Notion de 'mentalités' chez Marc Bloch et Lucien Febvre: Deux conceptions, deux filiations," *Revue de synthèse*, 111–12 (1983): 333–48. Further, on Bloch's and Febvre's program for reform, see H. Stuart Hughes, *The Obstructed Past: French Social Thought in the Years of Desperation, 1930–1960* (New York, 1966), 21–48.

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Economics; she hoped that in return historians could furnish evidence that, combined with theirs, might lead "to the establishment of laws of social behavior." Bloch's interdisciplinary links were informal rather than institutional and stretched beyond hers to matters of language and ritual; since history for him was, rather, "a science of change, of differences," it suggested possibilities, not laws.[61]

Here we come to a final contrast in their hopes for renewal. If both wanted to tell the history of social groups, Power's aims extended to the history of one group that Bloch never considered: the women of different social estates, who had interested her since her first research days at the London School of Economics. Moreover, Power was concerned (as Febvre was to be as well) about a social history that seemed too abstract, vague, and general. Her solution was *Medieval People*—a book in which the richly documented cast of characters, men and women both, mediate between Max Weber's ideal types and G. G. Coulton's everyday peasants.[62]

With these programs for change, how did Bloch and Power deal with their own teachers and with those who would come after them? To one group of forebears Bloch felt easily akin. His thesis, *Rois et serfs*, he dedicated to his father: "A mon père, son élève." Old Gustave Bloch, at seventy-two, was then finishing his last book, a study of the Roman empire. But, in the acknowledgments to *Rois et serfs*, a second name emerges, Christian Pfister, which, Bloch remarked, "would have been inscribed at the opening of these pages if . . . I had not felt I should dedicate my thesis to another of my masters who has been teaching me for an even longer time." Pfister was his professor at Paris and then his dean at Strasbourg: "How much I owe to his advice, to his inexhaustible goodness, and to his method and precision." When Pfister died some years later, Bloch once again placed him in the status of a father—Pfister and Gustave Bloch had taught courses in the same ill-lighted Paris classroom—adding that Pfister was one of those who had the right to have inscribed on his tomb "Veritatem dilexi" ("I loved the truth"), the very phrase that Bloch wished in 1941 might be engraved on his own ("Veritatem dilexit," "He loved the truth").[63]

[61] Among many places where Bloch and Power talk about the new directions rural history should take, see Marc Bloch, "La Vie rurale: problèmes de jadis et de naguère," *Annales d'histoire économique et sociale*, 2 (1930): 96–120; and Eileen Power, "On the Need for a New Edition of Walter of Henley," *Transactions of the Royal Historical Society*, 4th ser., 17 (1934): 101–16. Marc Bloch, "Pour une histoire comparée des sociétés européennes," initially published in the *Revue de synthèse historique* in 1928 and reprinted in Marc Bloch, *Mélanges historiques*, 2 vols. (Paris, 1963), 1: 16–40, especially 34; "Pour mieux comprendre l'Europe d'aujourd'hui," *Annales d'histoire économique et sociale*, 10 (1938): 62; "Que demander à l'histoire?," initially published in 1937 and reprinted in *Mélanges historiques*, 1: 6–8. Eileen Power, "On Medieval History as a Social Study" (Inaugural lecture delivered at the London School of Economics in 1933), *Economica*, n.s., 1 (1934): 13–29, especially 21.

[62] Tawney in *DNB, 1931–40*, 718; already in 1919, Alice Clark thanked Power for supplying her unpublished information about medieval working women for background to Clark's *Working Life of Women in the Seventeenth Century* (London, 1919), preface. Eileen Power, "The Position of Women," *The Legacy of the Middle Ages*, C. J. Crump and E. F. Jacob, eds. (Oxford, 1926), 401–33. M. M. Postan has added unpublished lecture material to this last essay for a posthumous book by Power, *Medieval Women* (Cambridge, 1975). Power's assessment of the limitations of Weber's ideal types is in "Medieval History as a Social Study," 19–20. Eileen Power, *Medieval People* (Boston and New York, 1924), preface. Interestingly enough, Bloch's much-appreciated teacher at Leipzig, Karl Bücher, was the author of a short study on medieval women, which Power used: *Die Frauenfrage im Mittelalter* (Tübingen, 1910).

[63] Marc Bloch, *Rois et serfs: Un Chapitre d'histoire Capétienne* (Paris, 1920), dedication and 14. Gustave Bloch, *L'Empire romain: Evolution et décadence* (Paris, 1922). Marc Bloch, "Christian Pfister: Les Oeuvres,"

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This doubling of historical collaboration and family ties occurs elsewhere. *Les Rois thaumaturges* of 1923 had no dedication—perhaps Bloch thought a book on the royal touch for scrofula would not make a good gift—but his two prefaces acknowledged, first, the contributions of Lucien Febvre and another Strasbourg colleague, "who will find so much of themselves in these pages," and, next, the aid of his physician brother and of his late father.[64] In many ways, the interdisciplinary team of the *Annales* appears to be a sodality of French brothers. (The main exception for Bloch was his wife, who served as his secretary, assistant, and reader of all his manuscripts. Even though numerous books by French women were reviewed in the pages of the *Annales*, only one female historian contributed an essay to it in the seventeen years of its existence.[65]) They were not always brothers who agreed: Febvre found Bloch's *Feudal Society* too sociologically deterministic; Bloch found Febvre's *Rabelais* too preoccupied with the scholarly thesis it was trying to refute rather than attending directly to "the historical reality" of the Renaissance writer. Nonetheless, in 1941, Bloch could write to Febvre about his manuscript on *The Historian's Craft* in the language that Montaigne had used about his dear friend La Boétie: "Of the many ideas [in this book], I wouldn't know how to decide in all honesty which come from you, which from me, and which from the two of us."[66]

Bloch also expressed gratitude toward forebears whose historical practice he did not follow in all regards, as in his dedication of *Feudal Society* to Ferdinand Lot,[67] but the real test for his relation to elders was in what he had to say about Langlois and Seignobos, that is, those whose systems he had been trying to supplant. His appreciation for Langlois had been courteous but restrained in the acknowledgments for his thesis, and in his obituary for his former teacher in 1929 are already

Revue historique, 172 (1933): 567. "Testament spirituel de Marc Bloch, le 18 mars 1941," *Annales d'histoire sociale*, 7, 1 (1945): iv.

[64] Marc Bloch, *Les Rois thaumaturges: Etude sur le caractère surnaturel attribué à la puissance royale particulièrement en France et en Angleterre* (Strasbourg and Paris, 1924), v–vii.

[65] Febvre on the role of "Madame Marc Bloch" (Simone Vidal Bloch) in Marc Bloch, *Apologie pour l'histoire ou métier d'historien* (hereafter, *Métier d'historien*) (Paris, 1949), 107; and see also further information in Weber, "Bloch," 75. Bloch did not acknowledge his wife's role in his existing publications, but Febvre said that, if he had lived, he would surely have dedicated a future work to her. Thérèse Sclafert, a professor of history at the Ecole des Jeunes Filles at Fontenay-aux-Roses, has an article in the first volume (1929) of the *Annales*. Bloch speaks with some approval of her *thèse d'état*, a geographical and economic study of *Le Haut-Dauphiné au moyen âge*, and other works by her in a review article in *Annales d'histoire économique et sociale*, 2 (1930): 98–99. Sclafert was still part of the *Annales* school in 1959, when her *Cultures en Haute-Provence: Déboisements et pâturages au moyen âge* was published in the SEVPEN series Les Hommes et la Terre. It may be that most of the women historians then publishing books were trained as archivists and were producing precisely that kind of rural history to which Bloch objected. The only other woman writing for the *Annales* was Lucie Varga, a refugee from Austria, who contributed an ethnographic study of an Austrian valley (*Annales d'histoire économique et sociale*, 8 [1936]) and an interesting account of the German support for Nazism (9 [1937]).

[66] Febvre's review of Bloch's *La Société féodale* in *Annales d'histoire sociale*, 2 (1940): 39–43; Bloch's 1943 letter to Febvre about *Le Problème de l'incroyance au XVI^e siècle: La Religion de Rabelais* in *Annales d'histoire sociale*, 7, 1 (1945): 28–29; Bloch's appreciation of Febvre in May, 1941, *Métier d'historien*, vii. On their disagreement, see the interesting article by Burguière, "La Notion de 'mentalités.'" Bloch planned a characteristic double dedication to *Métier d'historien*: it was to be dedicated to the memory of his late mother, and the letter to Febvre was to be included "by way of dedication" ("en manière de dédicace").

[67] Marc Bloch, *La Société féodale*, 2 vols. (Paris, 1939): "A Ferdinand Lot, Hommage de respectueuse et reconnaissante affection." See Bloch's review of Lot's three volumes, *Les Invasions barbares et le peuplement de l'Europe*, in *Annales d'histoire économique et sociale*, 10 (1938): 61–63.

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the reservations (somewhat unfair ones, we may note) that Bloch later developed in *The Historian's Craft*. That enormous erudition, all those useful studies of royal documents and summaries of medieval tales, and yet an ironic detachment from discovering the great and varied "currents of human life" hidden within them. Numerous implications for social structure and economics in his source materials, yet always a political angle, even when he was talking about the origins of the nobility. Outwardly a rigorous judge of fact, Langlois carried a well of skepticism inside, so Bloch thought; history had become for Langlois "an aesthetic game" of sorting documents, and he had renounced the delicate task of interpreting them.[68] In a 1942 letter to Febvre, Bloch put the contrast more simply. Certainly, poor Father Seignobos was no imbecile, or Charles V either. "But how far we are from them! If it were only in our solutions or efforts at solutions, that would be nothing. But it's even in our problems!"[69]

In *The Historian's Craft*, Bloch found a good way to state that difference. The query with which it opens—"Tell me, Daddy, what is the use of history?"—is identified as a question that Seignobos had found "idle"; Seignobos' view that "it is useful to ask oneself questions, but very dangerous to answer them" is found lacking in courage before the challenge of discovering what kind of a "human science" history might be. Still, he calls Seignobos his "cher maître," says how much his education owed to the teaching and scholarship of both him and Langlois, and goes on: "But they not only taught us that the historian's first duty is to be sincere, they also did not conceal the fact that the very progress of our studies is based on the inevitable opposition ('la contradiction nécessaire') between generations of workers. Therefore I shall be keeping faith with their teaching in criticizing them most freely wherever I deem it useful, just as I wish that one day my students will criticize me in their turn."[70]

[68] Marc Bloch, *Rois et serfs*, 14; obituary for Charles-Victor Langlois in *Annales d'histoire économique et sociale*, 1 (1929): 583–84. Bloch was exaggerating his differences from his teacher: Langlois' *Lectures historiques* of 1912 includes essays on serfs with material drawn from literary sources. His retelling of medieval stories, though it added no commentary whatsoever, was motivated by the desire to go beyond political and administrative history and find something of "the sentiments of people of the Middle Ages" (Langlois, *La Société française au XIII^e siècle d'après dix romans d'aventure* [Paris, 1904], i). Both social and intellectual topics were included by Langlois and Seignobos in their schematic "general classification of historical facts" (*Introduction aux études historiques*, 201–03).

[69] Bloch to Febvre, 17 August 1941, in *Annales d'histoire sociale*, 7, 1 (1945): 31. "Charles V" was the nickname Langlois' former students gave Charles-Victor. Bloch also included Durkheim in this distancing—"Durkheim n'était, certes, pas un imbécile"—and took issue with his sociological approach in *Métier d'historien*. What had once been a fertile influence was now outmoded and too abstract. On Durkheim's influence on Bloch, see Perrin, "Oeuvre historique," 184; and Burguière, "Mentalités chez M. Bloch," 338–39.

[70] Bloch, *Métier d'historien*, ix, xv–xvi, and 109 n. 1. For the most part, I follow here the translation by Peter Putnam in Marc Bloch, *The Historian's Craft*, with introduction by Joseph R. Strayer (New York, 1953), 3, 15–17. Bloch added somewhat disingenuously (3 n. 1) that the Langlois and Seignobos *Introduction aux études historiques* is "justly famous" and that his book, "arranged upon a different and, in certain of its parts, a much less fully developed plan does not by any means pretend to replace [it]." Interestingly enough, at the very same time that Bloch was beginning his work on *The Historian's Craft*, Charles Seignobos, then aged eighty-seven, was writing his old friend Ferdinand Lot about a new book he hoped to do on the "principes de la méthode historique" (letter dated June, 1941). He thought that, for a quarter of a century, no serious reflection had been done on historical method (H. E. Barnes's book he found mediocre, and he said nothing of Bloch, Febvre, and the *Annales*). The project he

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This is the stance—asserting the dialectic of change and his own fallibility—that allowed Bloch to maintain the distinction between history's two bodies. Thus he could demolish 900 pages of pretentious publication on the Abbey of Saint Denis by archivist Germaine Lebel as careless reporting of a small number of note cards—"and this is how one puts books together!"—partly because he felt he had been frank about the limitations of his own work. He told readers of his *Rois thaumaturges* that he was publishing it with its omissions rather than keeping it forever in folders and would be grateful if they would send him corrections and additions. His *Caractères originaux* he presented as a set of hypotheses about French rural history, adding that, "when the time comes for my own work to be superseded by studies of deeper penetration, I shall be well rewarded if confrontation with my false conjectures has made history learn the truth about herself." *Feudal Society* brought with it a reminder of "the uncertainty of our state of knowledge"; but, since history was "a science in movement," he hoped he would whet the appetite of some young researchers.[71]

Bloch's actual word here was not "researchers" but "workers," "travailleurs." It was a word he preferred, a word used by Febvre and Bloch in the first number of the *Annales*, a word he used against Friedrich Meinecke in his review in 1939 of *Die Entstehung des Historismus*. "'Historicism' or historians' work?" Bloch asked, and, while admiring some of Meinecke's characterizations, he expressed astonishment at a view of historical thought that saw it reaching perfection in Ranke's day with nothing to come after, and at a presentation of the "spirit" of historical science that did not consider the development of its techniques. It was, of course, as an artisan that Bloch was to present himself in *The Historian's Craft*, a carpenter's journeyman, who knew his tools and had thought about his tasks.[72]

It is harder to uncover Eileen Power's self-image as historian, for in print she was reserved about herself and her connections. She seems to have saved her own voice for commenting directly on her wool traders, nuns, and peasants, while her discussions of scholarly method were impersonal. Surely, it is significant that from the first she called herself Eileen Power on her title pages, rather than following the contemporary English practice of using only initials; E. E. Power might have led her to be referred to, as was Dr. S. Thrupp in some of her first reviews, as "he."[73]

In regard to her two teachers, Coulton and Langlois, she was less directly critical of them than was Bloch and slyer in letting them know that she had struck off on her own path. Coulton, who was thirty-one years her senior, she teased: "I

outlined has the same structure and argument as his 1891 book with Langlois (R. Fawtier, "La Dernière lettre de Charles Seignobos à Ferdinand Lot," *Revue historique*, 210 [1953]: 1–12).

[71] Marc Bloch, "Apologie pour le travail utile: A propos de deux livres sur Saint-Denis," *Annales d'histoire économique et sociale*, 9 (1937): 80–85. *Rois thaumaturges*, vi; *Les Caractères originaux de l'histoire rurale française* (Oslo, 1931), viii, trans. J. Sondheimer, *French Rural History*, xxii; *Société féodale*, 1: 8.

[72] "A nos lecteurs," *Annales d'histoire économique et sociale*, 1 (1929): 1; Marc Bloch, "'Historisme' ou travail d'Historiens?," *Annales d'histoire sociale*, 1 (1939): 429–30; *Métier d'historien*, xvii.

[73] Her full name was Eileen Edna le Poer Power. For reviews of Sylvia Thrupp's contribution to *Studies in English Trade in the Fifteenth Century* in which Dr. S. Thrupp is thought a man: V. J. Galbraith in *Economica*, n.s., 1 (1934): 349–50; and M. Bloch in *Annales d'histoire économique et sociale*, 6 (1934): 317.

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observe," she wrote to him in 1922, "that *The New Statesman* in its Autumn Booklist has entered to be what I presume to be my nuns under the title G. G. Coulton, *Studies in Medieval Life and Thought*: a view of the comparative importance of mother to obstetrician, which looks like 'another injustice to women!'" Her later reviews of his work were favorable on the whole, spelling out what he did and did not do, and chiding him lightly for "the bee in his bonnet," that is, the polemical concerns that sometimes got in the way of his honey. In her inaugural address on social and economic history, she praised his "illuminating studies," noting that they had different goals from those she was advocating, but "social history is a wide subject."[74] As for Langlois, who must have thought her *Medieval People* departed too far from the intention of its sources and was too much of a popular "history book," in 1928 Power dedicated to him her English translation and annotation of a fourteenth-century text, saying that she would "always be grateful for having been his pupil." But what was the text? *Le Ménagier de Paris*, the domestic instructions of a Parisian householder to his young wife—a far cry from the sober moral treatises that Langlois had summed up in his collections.[75]

As for her women teachers, Power did not mention them, and one wonders whether she did not feel as motherless in the university world as she had been in her childhood. The forebears that emerge in her writing are distant ones: Madame Eglentyne, Chaucer's worldly prioress, whose equivalent she kept finding in her sources, and the learned and independent Christine de Pizan, who often serves as her commentator on the situation of medieval women.[76] Close at hand, however, she had sisters, not only the biological sister with whom she wrote a book on children but her friends from Girton, whom she thanked in *Medieval English Nunneries* and in later books for "faithful criticism." *Medieval People* was dedicated to "my colleagues and students at Girton College Cambridge, 1913–1920," followed by a quotation from *Piers Plowman*, in which William Langland celebrates the cloister and the school as places for ease of soul, books, "buxomnesse," and love for those who learn.[77]

[74] Power to Coulton, 9 October 1922, attached to her *Report to the Trustees*. She also acknowledged Coulton's help in the preface to *Medieval English Nunneries*, but he is not mentioned in later prefaces. Power's review of vol. 3 of Coulton's *Five Centuries of Religion* is in *Economic History Review*, 8 (1936–37): 87–89; Coulton, *Fourscore Years*, 265; Power, "On Medieval History as a Social Study," 17.

[75] *The Goodman of Paris (Le Ménagier de Paris): A Treatise on Moral and Domestic Economy by a Citizen of Paris (c. 1393)*, trans. with introduction and notes by Eileen Power (London, 1928), dedication to Charles-V. Langlois. Langlois had summed up moral treatises, such as one by Raymond Lull, in *La Vie en France au moyen âge d'après quelques moralistes* (Paris, 1908). On Langlois' negative attitudes toward "history books" and writing for the public rather than publishing "original documents . . . without mixing anything [of the historian] in them," see Keylor, *Academy and Community*, 84–86, 178–80. His retelling of thirteenth-century tales, without commentary or interpretation, was aimed at "a lettered public, not medievalists by profession" (*Société française*, xiii).

[76] A manuscript image of Madame Eglentyne is on the cover of *Medieval English Nunneries*, and the book closes with Chaucer's verses. Power's chapter on nuns in *Medieval People* is based on Madame Eglentyne as a type. Christine de Pizan is cited in both these texts but figures centrally as a commentator and source in Power's essay on "The Position of Women" and in *Medieval Women*, the posthumous book edited by M. M. Postan.

[77] Power collaborated with her sister Rhoda Power on a book entitled *Boys and Girls of History* (Cambridge, 1926). *Medieval English Nunneries* was dedicated to her Girton friend, M. G. Jones. Jones is thanked again in the preface to *Medieval People*, as is also Miss H. M. R. Murray of Girton. Jones later aided Michael Postan in editing Power's Ford lectures for her posthumous book, *The Wool Trade in*

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In transferring Langland's male "hevene on this erthe" to Girton, Power carried within her an idealized past that presumably had a role to play in her transformative life in London. A sustaining role, perhaps, as she knit her solidarities with Tawney and then with Michael Postan, ten years her junior, who started as her research assistant, was later her collaborator, and for the last three years of her life her husband.[78] But, more important, the idealized Girton past may have had a relativizing role: it may have helped remind her that, although she could rightly condemn, for instance, A. Birnie's *Economic History of the British Isles* for being hopelessly out of date, misleading, full of exploded views about "the manor" and medieval loans, nonetheless, she could never speak as History incarnate. As Bloch's view of history as changing through generational conflict qualified the absoluteness of his own scientific claims, so Power's loyalty to different settings for producing history added capaciousness to her vision.[79] "Social history is a wide subject"; so is history more generally.

Eileen Power died so unexpectedly that she had little occasion to reflect on her succession. Michael Postan carried on the projects they had shared, and many younger historians in England and abroad mourned her. One was Sylvia Thrupp, who dedicated her *Merchant Class of Medieval London* to her memory. Thrupp recalls an exchange they had in London. Power said, with perhaps some unease, "You want to follow my example." Thrupp answered, "We're moving in the same direction." What better tribute to a historian than that she could elicit such an independent response?[80]

Marc Bloch wrote in his last years under the shadows of war, occupation, anti-Semitism, and death. He had his moments when he wondered whether his work would have an issue. Then, in late 1942, we hear that core commitment of the historian that goes beyond credit, beyond renown, and beyond succession. Several months before he joined the Resistance, he wrote a letter to his son Etienne, the one who had once asked him what history was for: "Afternoons I'm working especially on my book (*Historian's Craft* seems to me a better title than *Apology for*

English Medieval History (Oxford, 1941), vii. Mary Glwadys Jones was to publish books on eighteenth-century charity schools (1938) and on the writer-moralist Hannah More (1952). The quotation from *Piers Plowman* is from Book 10.

[78] Born in Bessarabia in 1898 and educated at the University of Kiev, Postan had come from the Soviet Union to England after the revolution. He was already a research assistant for Power at the time of *Tudor Economic Documents* in 1924 (1: viii). He was lecturer in history at University College London from 1927 to 1931, lecturer in economic history at the London School of Economics from 1931 to 1935, and then lecturer and finally professor at Cambridge. He and Power were married in 1937 and had homes in both Cambridge and London.

[79] See Power's review of Birnie in *Economic History Review*, 8 (1937–38): 82–83. Power's sense of history as capacious was surely enhanced by her travels; she returned to the Far East for a second time in 1929.

[80] Conversation with Sylvia Thrupp, 4 December 1987. Sylvia Thrupp, *The Merchant Class of Medieval London (1300–1500)* (Chicago, 1947), vii; paperback edn. (Ann Arbor, Mich., 1962), dedication. Among those scholars who acknowledged Power's aid during her life were her student Ivy Pinchbeck in *Women Workers and the Industrial Revolution, 1750–1850* (London, 1930), preface, and H. S. Bennett, *Life on the English Manor, 1150–1400* (Cambridge, 1937), preface. A moving obituary of Power was written by Philippe Wolff, one of the young historians she had helped when he visited England. It appeared in the issue of the *Annales* primarily devoted to the work of Marc Bloch and said of her work, "Car l'esprit dont témoigne toute sa production est celui même qui nous anime"; *Annales d'histoire sociale*, 7, 2 (1945): 127–28.

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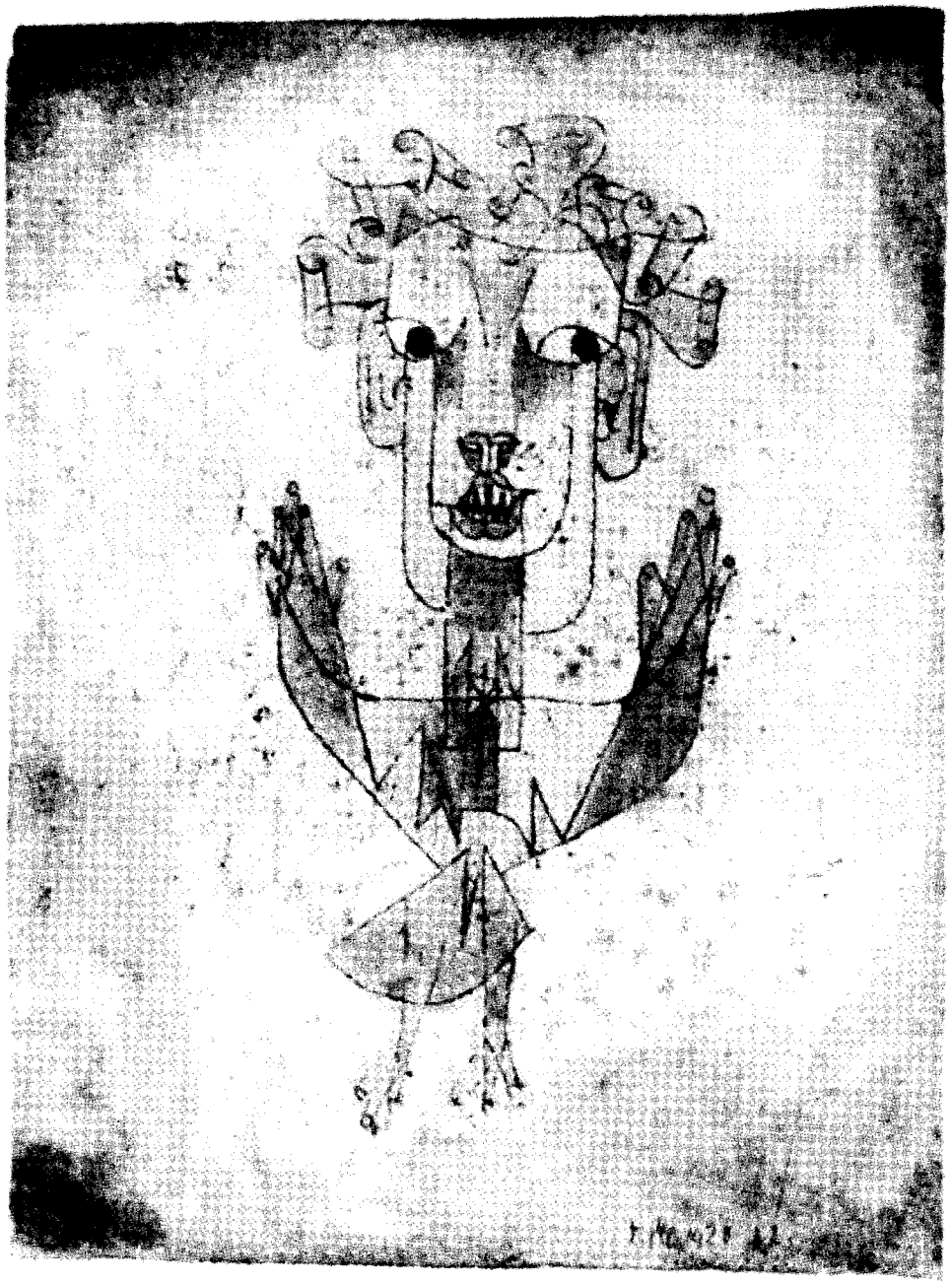


ILLUSTRATION 6: "Angelus Novus," a watercolor by Paul Klee, 1920. Reproduced by permission of the Israel Museum, Jerusalem. The black-and-white version here is from Wilhelm Hausenstein, *Kairuan, oder Eine Geschichte vom Maler Klee* (Munich, 1921).

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History. What do you think?). It's going very slowly, but at least it's going, and though I have my usual doubts, it doesn't seem without interest. When will it ever be finished? When will it ever be able to appear? Really, to work now is to work for the Muse."[81]

I AM SURE THAT FELLOW HISTORIANS have recognized here the feelings and struggles of these forebears: the desire for rightful credit and indignation against thieves, the resentment of rivals and the appetite for renown, the anxiety of influence—both the influence of one's teachers and the influence over one's students and successors. To practice as historians, in a discipline committed to finding and making truthful sense of the past, they had to moderate these claims or at least put them to work for a higher cause. And so do we as well. Some of their strategies are deeply embedded in their own time: none of us, in our contemporary frenzy for publication, would seek revenge like Pasquier through withholding our works. But it could help to imagine one's work as a gift, taken from a plentiful storehouse. Macaulay's oak wreath and Roman poses are out of fashion, as is her ardent belief in perfectibility, and few of us would dare append the passions of *Our Own Lives* to every posthumous edition of our works the way Hume did. But laughing at ourselves does help to undermine any pretentions to sovereignty, as does recalling the larger concerns of an international historical community. Power and Bloch offer us overlapping models for critical innovation, a generosity of vision, and an admission of fallibility, Bloch reminding us finally of that bedrock of loneliness when we write only for our inner muse.

I have been thinking how I might give an image to History that would suggest the complexity, commitment, and multiple vision that I believe must be at its heart. For a time, I considered adopting the Angelus Novus, the new angel of Paul Klee's watercolor (Illustration 6), which Walter Benjamin acquired in 1921 and kept with him until he fled from Paris in 1940. You will recall that Benjamin, in one of his last aphorisms, made this the angel of history: "His eyes are staring, his mouth is open, his wings are spread . . . His face is turned toward the past. Where we perceive a chain of events, he sees one single catastrophe which keeps piling wreckage upon wreckage and hurls it in front of his feet. The angel would like to stay, awaken the dead, and make whole what has been smashed. But a storm is blowing from Paradise . . . [It] irresistibly propels him into the future to which his back is turned, while the pile of debris before him grows skyward. This storm is what we call progress."[82] What I like about the Angel of History is that, unlike the Muse, an angel has no sex in theology, even though the German language gives it a gender; and Benjamin's aphorism puts at the core of history an eternal

[81] Marc Bloch to Etienne Bloch, 13 September 1942, in E. Bloch, *Marc Bloch*, 16.

[82] Walter Benjamin, "Theses on the Philosophy of History," in *Illuminations*, Harry Zohn, trans. (New York, 1969), 257–58. On the interpretation of this text and Benjamin's historical views, see O. K. Werckmeister, "Walter Benjamin, Paul Klee, and the Angel of History," *Oppositions*, 25 (Fall 1982): 103–25; Richard Wolin, *Walter Benjamin: An Aesthetic of Redemption* (New York, 1982), 48–63; and Michael Jennings, *Dialectical Images: Walter Benjamin's Theory of Literary Criticism* (Ithaca, N.Y., 1987), chap. 2.

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tension—between wholeness and fragmentation—and a multiple vision: ours, the Angel's, and that of the wind from Paradise. But the New Angel is not quite right; it is too unchanging, too sober. My image of History would have at least two bodies in it, at least two persons talking, arguing, always listening to the other as they gestured at their books; and it would be a film, not a still picture, so that you could see that sometimes they wept, sometimes they were astonished, sometimes they were knowing, and sometimes they laughed with delight.

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The Politics of Divorce in France of the Belle Epoque: The Case of Joseph and Henriette Caillaux

EDWARD BERENSON

FOR SOME TIME NOW, HISTORIANS OF MEDIEVAL and early modern Europe have been using the records of trials, both religious and secular, to explore cultures and societies obscured by the absence of any other written records. This kind of work, made famous by Emmanuel Le Roy Ladurie, Natalie Zemon Davis, and Carlo Ginzburg, formed the vanguard of the shift from an economic and social history miscast as *histoire totale* to what could be called *histoire épisodique*, an approach to the past through one exemplary event or person. The shift from social to anthropological history has not been without its problems, but it has enabled us to glimpse elements of religious belief, family life, sexual practices, and political power that we might otherwise have missed.

Curiously, these new episodic techniques have been limited almost exclusively to medieval and early modern history. It may be that this kind of work has been so influenced by cultural anthropology—a discipline oriented toward non-modern societies—that the emerging anthropological history has almost naturally devoted itself to earlier periods. I hope to show, however, that historians of the recent past can also use one or more key trials to gain entry into spheres of culture and politics whose vital meanings tend to escape other methods of enquiry. In these spheres, culture and politics often illuminate each other by dissolving the largely artificial barriers between public and private, male and female, *grande histoire* and *petite histoire*. France, moreover, is particularly suitable for episodic analysis, because its centralization brings together government, commerce, the high courts, intellectual life, and the press in ways that permit a singular event involving all these realms to yield new historical insights.

The trial considered here is a spectacular murder case, proclaimed the "Affaire Caillaux" by the giants of Parisian journalism.[1] The case riveted the capital's

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[1] The Caillaux Affair has long fascinated historians of the Belle Epoque. For recent accounts of the trial, see René Floriot, *Deux femmes en cour d'assise* (Paris, 1966); Paulette Houdyer, *L'Affaire Caillaux . . . ainsi finit la belle époque* (Les Sables-d'Olonne, 1977); Benjamin F. Martin, *The Hypocrisy of Justice in the Belle Epoch* (Baton Rouge, La., 1984).

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attention during the final week of July, 1914, and diverted almost everyone from the looming threat of war. Henriette Caillaux, wife of France's controversial ex-premier Joseph Caillaux, stood trial for murdering her husband's most relentless adversary: Gaston Calmette, editor of *Le Figaro*. Henriette Caillaux did not deny the shooting, and several witnesses testified that she indeed had done it. But after eight full days of testimony, the jury needed only fifty minutes to reach a verdict: not guilty.

For months prior to the shooting, the right-wing daily *Le Figaro* had subjected Joseph Caillaux, leader of the Radical party and the nemesis of nationalists, to a campaign of libel and character assassination that was dirty even by the decidedly lax standards of the Third Republic. This campaign reached its low point on 13 March 1914, when Calmette violated the one canon taken more or less seriously by Parisian pundits: the stricture against delving openly into a politician's private life. The front page of *Le Figaro*'s March 13th issue featured the facsimile of an intimate letter Joseph Caillaux had written some thirteen years earlier to Berthe Gueydan, the mistress who later became his first wife (Henriette was his second). The letter contained evidence of political double-dealing on Joseph's part and ended with an embarrassingly amorous closing, "Ton Jo" ("Your Joey"). By reproducing this document, the editor Calmette added ridicule to his trumped-up outrage against Joseph Caillaux. But, more important, the letter touched Joseph and Henriette where they were most vulnerable in the moral climate of 1914: their checkered personal lives.

Joseph Caillaux was known for his *liaisons élégantes*, a celebrated divorce, and twice marrying mistresses who had left their husbands to be with him. Henriette Claretie, likewise, had violated her own marriage and that of another woman by her relationship to Joseph Caillaux. As two of France's most public figures in the years before 1914, the Caillauxs found themselves open to charges of hastening the decay of France's moral life, of transgressing against the duties of marriage and family. Their vulnerability on personal and ethical grounds stemmed from a conservative revival that marked pre-1914 France, a revival that found expression not only in a new nationalism but in a backlash against the secular republic's moral relativism and against its apparent challenge to the traditional family.

By 1914, the Catholic church and other conservative groups had gained considerable strength from a politically and religiously charged debate over marriage, divorce, and the family that began in 1884 with a law permitting divorce and took on new intensity at the turn of the century.[2] Conservatives may have been

[2] Political labels like "conservative" are by their nature imprecise, but since we are condemned to use them, definitions help, especially definitions that acknowledge "politics" as involving much more than elections and parties, doctrines and dogmas—the traditional concerns of political historians. The realms of culture and morality are central to any understanding of politics. For this reason, evaluation of an individual's position must extend to views of family, gender, sexuality, education, and the like. So, by "conservatives," I mean those who tended to agree with the church on moral and cultural issues, who extolled the "traditional" family and aimed to preserve the woman's customary place in it. Intimately related to these beliefs was the conservatives' hostility to the democratic and egalitarian trappings of the Third Republic, their hatred of the revolutionary legacy of 1793. They did not necessarily oppose the republic as a form of government but rejected the "excesses" to which 1789 had been prone. Conservatives were nationalistic and more or less imbued with Germanophobia. They

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defeated in the Dreyfus Affair, but they staged a rapid comeback after 1900, when rekindled concerns about French degeneration and German prowess allowed them to focus new attention on the family. France's moral decay, commentators claimed, had spread to the family itself, an institution progressively weakened by feminism, individualism, and divorce. Depopulation was the inevitable result, so much so, they said, that France might soon be overwhelmed by a fecund German empire.[3] In this context, efforts first to establish legal divorce and then to allow it on demand provoked a traditionalist backlash that idealized the family of yore and gave conservatives a cultural issue potent enough to engender a robust political resurgence. By raising the family question, anticlerical republicans enabled conservatives to shift their challenge to the radical republic from political to cultural grounds—that is, from a terrain on which rightists enjoyed little support after 1900 to one from which they could build widespread public assent. In this setting, Calmette's ability to open the Caillauxs' private life to public inspection menaced them perhaps even more than the editor himself realized.

At her trial, Henriette Caillaux testified that the fear that her past moral violations would be revealed had upset her to the point of momentary derangement, to a loss of conscious will (*volonté*) that produced an unconscious act of violence.[4] Whether or not her explanation was sincere, the ferocity of the conflicts in this period over marriage, divorce, and the family made it plausible. In the Belle Epoque, it was one thing for a man to be accused of transgressions against marriage and the family, quite another for a woman of high bourgeois standing. To be a *bourgeoise* was by definition to resist calls for female emancipation and relaxed moral constraints.[5] And, for this reason, Calmette's ability to prove Henriette had not resisted threatened her not just with social disgrace but with loss of social identity. How could she claim status as a proper *bourgeoise* if everyone knew she had violated the conventions of behavior such status required? Paradoxically, of course, it was she herself who brought these transgressions into the open by committing an act that led to one of France's most public trials since the Dreyfus Affair. Even so, Henriette Caillaux's ability to portray herself as acting—however irrationally—in the name of love, as impelled by fragile nerves and feminine emotions, earned her the court's indulgence and, ultimately, its acquittal.

believed in preparing seriously for the eventuality of war. And they loathed socialism and anything that smacked of "collectivism." They could nonetheless be enlisted for projects to ameliorate working and living conditions for the poor. It was, of course, possible to be conservative on some issues and less so on others—especially when moral and cultural concerns were taken into account. But the further people were to the right of Radicalism, the more they fit the above definition on most counts.

[3] The French were obsessed with depopulation during this period, producing no fewer than eighty-two books and scores of articles on the subject between 1890 and 1914. These statistics come from Steven C. Hause, *Women's Suffrage and Social Politics in the French Third Republic* (Princeton, N.J., 1984), 22. See also John C. Hunter, "The Problem of the French Birth Rate on the Eve of World War I," *French Historical Studies*, 2 (Fall 1962): 490–503; Karen Offen, "Depopulation, Nationalism, and Feminism in Fin-de-Siècle France," *AHR*, 89 (June 1984); Robert Talmy, *Histoire du mouvement familial en France (1896–1939)*, vol. I (Aubenas, 1962), 41–65. Books from the era are too numerous to list here, but the most prominent authority on the subject was Dr. Jacques Bertillon, author of *La Dépopulation de la France* (Paris, 1911).

[4] Transcript, *Le Temps*, 21 July 1914.

[5] See Anne Martin-Fugier, *La Bourgeoise: Femme au temps de Paul Bourget* (Paris, 1983), 82–92.

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Her husband would benefit from no such sympathy. Joseph Caillaux emerged from the trial a political outcast, *her* case having prepared the way for *his* conviction in 1918 on the groundless charge of "intelligence with the enemy."[6] The right had not been able to defeat Joseph Caillaux through traditional political means, but attacks against his personal life ultimately bore fruit. For this reason, the Affaire Caillaux was as much about the defendant's husband as the defendant herself. And though much more remains to be said about Henriette's testimony, I want to turn instead to the testimony of Joseph and in particular to that of his first wife, Berthe Gueydan.

THE CAILLAUXS HAD ACCUSED MME GUEYDAN of giving Calmette three embarrassing personal letters that were to figure heavily in the case against Henriette Caillaux: the "Ton Jo" letter plus two others Joseph had written Henriette while he was still married to Berthe. The Caillauxs' accusation was plausible since the first letter had been written to Berthe, while the others had fallen into her hands toward the end of her marriage to Joseph Caillaux. The Calmette family lawyer, Charles Chenu, hoped not just to prove this allegation false but to use it to remind the jury of the Caillauxs' prior adultery—a transgression that had resulted in Berthe Gueydan's abandonment and divorce.[7] The pair of letters from Joseph to Henriette, ultimately read in open court, served the prosecution well; nothing could have been more incriminating than Joseph's almost adolescent ardor for his then-mistress Henriette. "There is only one consolation," he wrote in September, 1909, "one lone succor [in life]; to think of my little one, to see her in my arms as at Ouchy . . . (God, what delicious moments!)." Joseph's explicit closing armed his opponents all the more: "A thousand million kisses all over your adored little body."[8]

Meeting in court three years after the collapse of their marriage, Joseph and his first wife bristled in each other's presence. So hostile was their exchange that an uninformed spectator might have believed it was their defunct marriage at issue, not the murder of one of Paris's leading newspapermen. The focus on the first marriage was especially evident in that Joseph Caillaux's controversial testimony two days earlier had already seemed to direct the proceedings more toward him and his personal history than toward Henriette and her crime. When Berthe Gueydan began her testimony, it was almost as if the actual defendant had ceased to exist.

As a witness, Berthe did not impress. She appeared cold, haughty, brittle, and her refusal to cooperate with the judge alienated spectators and journalists alike. Mme Gueydan, reported *Le Petit parisien*, "assumed an aggressive stance from the

[6] For details, see Jean-Denis Brédin, *Joseph Caillaux* (Paris, 1980), part 3.

[7] France's judicial code provided not simply for a government prosecutor to press the state's murder charge but gave the victim's family (the *partie civile*) the right to courtroom representation as well.

[8] See the transcript of the trial published in successive issues of *Le Temps* (and most other Paris dailies), July 21–29. Calmette's family had brought the distinguished conservative barrister Charles Chenu into the courtroom in accordance with French law, which permitted the aggrieved party its own representation in criminal cases.

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start and she maintained it, nuanced here and there, for the duration."[9] Still, observers in the courtroom could not help but feel some sympathy for her over the way she had been treated by this powerful man. Joseph Caillaux had begun his dalliance with Henriette Claretie less than a year after marrying Berthe.[10] And beyond the lies that had not fully disguised his adultery, he had humiliated his first wife by neglecting the social gatherings she hosted. Worst of all, Joseph had conspired with his mistress to hold off divorcing Berthe until the political moment was right. As he had written Henriette in one of the purloined letters, later to be read in court, "I will be able one day to reclaim my freedom [from Berthe], but in no case can I make a move before the elections."[11]

There was little Joseph Caillaux could say in his defense, so, skilled politician that he was, he took the offensive. This former prime minister was nothing if not audacious, for he dared not only to launch a public attack against the woman he had wronged but to seek justification for his past behavior in the Belle Epoque's masculine images of the New Woman. In a courtroom packed with journalists and politicians, a courtroom from which women spectators had been barred, Joseph told Berthe—for the public record—why she had been unfit to be his wife. "Between a man to whom everyone grants authority, vigor, and power," said Joseph Caillaux, turning to confront Mme Gueydan directly, "and you in whom these qualities are overdeveloped as well, it was impossible that things would last." Joseph told Berthe and the world that she was too forceful, too strong-willed, and too intelligent for his needs. How, he asked, could he have remained with her given their "conflict of natures at once opposed to each other and too similar?"[12]

With Henriette, Joseph continued, everything was different, balanced, natural; for, unlike Berthe, she was a "bourgeoise . . . from my sphere in society . . . with a character and a nature to which I could attach myself."[13] In the cultural climate of 1914, Joseph Caillaux had no trouble admitting that what he needed was the kind of wife most of his male compatriots would have agreed was a "real" woman—one who was soft, quiet, and retiring, who shared none of the quintessentially masculine qualities "overdeveloped" in himself.[14]

Joseph Caillaux, a man who seldom admitted to mistakes in the political realm, readily confessed the error of marrying a woman too much his intellectual peer. And because he had done so, Joseph told the court, he agreed magnanimously to pay for his fault. As part of the divorce settlement, Joseph gave Berthe the elevated sum of 18,000 francs a year plus investments worth 210,000 francs. The two figures, claimed the former premier, represented half his fortune. When Berthe objected that no amount of money could repair the emotional damage he had done her, his response was so arrogant as to provoke loud protests in the courtroom: "Permit me to remind you," he announced, addressing his former wife once again,

[9] *Le Petit parisien*, 24 July 1914.
[10] Henriette was married at the time to a literary critic of modest reputation named Léo Claretie.
[11] These personal letters became part of the case's transcript. See *Le Temps*, 25 July 1914.
[12] Transcript, *Le Temps*, 24 July 1914.
[13] Transcript, *Le Temps*, 24 July 1914.
[14] The refrain in a song from the era went: "Ce qu'il faut/C'est une femme douce." See Bernard Amoudru, *De Bourget à Gide: Amour et famille* (Paris, 1946), 42.

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"that you possessed not a *centime* when you came into my house."[15] It is important to note that only his pride in economic superiority aroused indignation from the audience, not his candid desire for intellectual and emotional precedence over the woman he married. This aspiration seemed so natural to the male audience that it produced not a ripple of dissent.

But, clever as these efforts were, none of them could explain away his history of unfulfilled promises and marital deceit. Especially unmoved were the growing number of conservative commentators who identified Joseph Caillaux's moral failings with his presumed political corruption. For them, divorce dissolved the family in the same way that radical republicanism dissolved the nation. Paul Bourget, the popular novelist, wrote in his regular newspaper column that one of the deepest and most tragic lessons of the Affaire Caillaux concerned the problem of divorce. To him, the testimony in the case "strikingly proved that divorce solves nothing, appeases no one, that it soothes no suffering, dampens no grief." Things were different, he wrote, "when marriage was indissoluble . . . for couples knew that the chain linking them was unbreakable, and even those in difficult marriages did their best to accommodate themselves to life's reality." Such unfortunates stayed together not for themselves but for the good of the community, since all societies need the assurance that fundamental relationships will last, that some things are permanent. The durability of families was crucial, Bourget maintained, for political reasons as well. He had written earlier that "for something to be solid and durable it must be natural, and what is natural in politics is the family. The family must be the basis for all our institutions." Bourget's concern over threats to the family explains why his consideration of the Caillauxs turned on the question of divorce. "More than any other of the so-called modern reforms," he concluded, divorce had uprooted French society from its traditions by absolving people of the belief "that certain things are inevitable and must be accepted."[16]

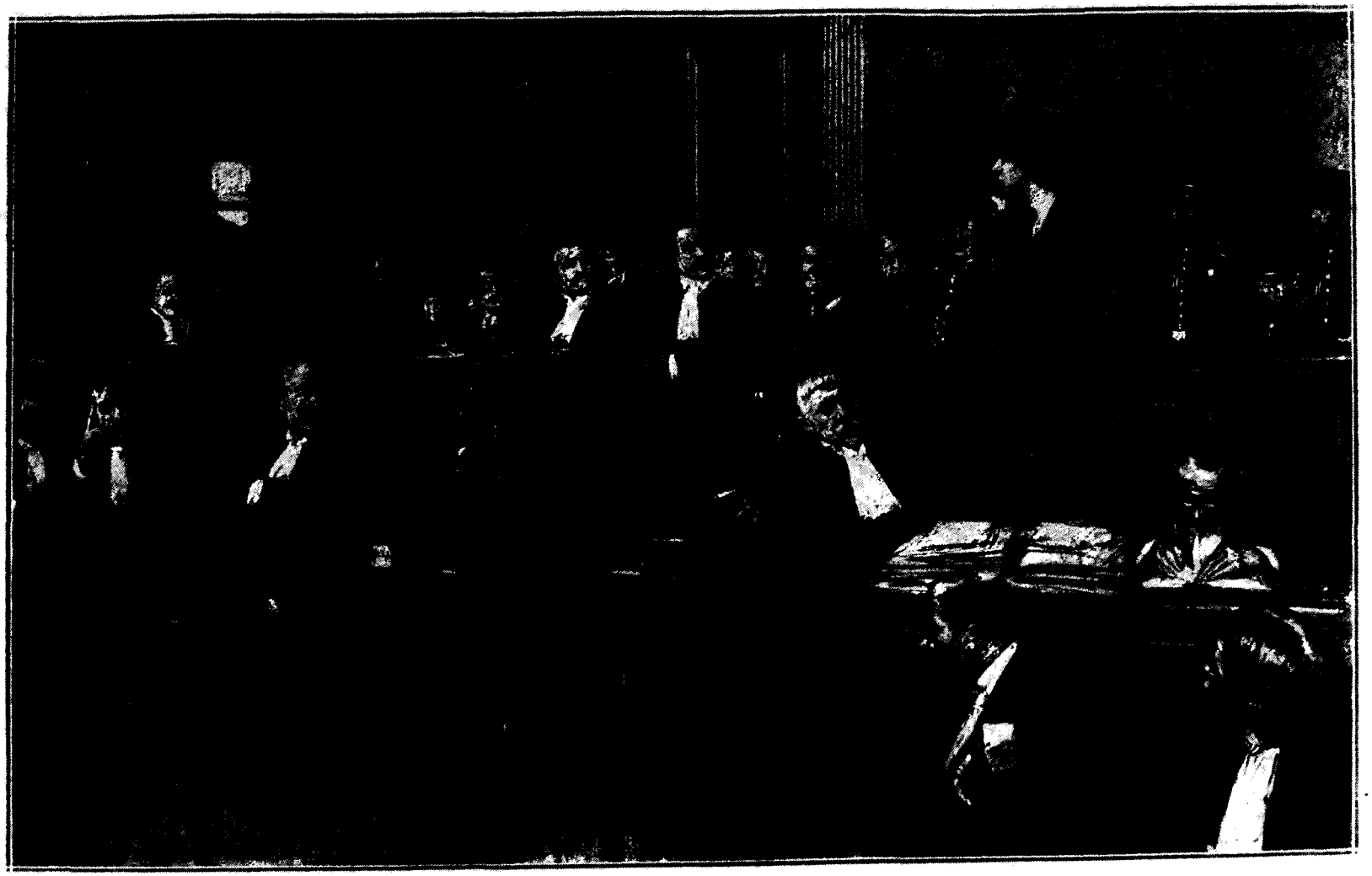
Like Bourget, but in more strident terms, the right-wing journalist Guy de Cassagnac linked the Caillauxs' divorces with a wider moral and political breakdown. Writing in *L'Autorité*, Cassagnac declared that "ours is a lamentable epoch, one in which divorce has overturned the laws of society and destroyed all respectability. It has precluded a peaceful home life and created a fearful chaos among women, children, and husbands . . . [It] ignites scandal and stirs up hatred." More than any other recent event, the Affaire Caillaux reminded Cassagnac of the evils of divorce. "When I heard Mme Caillaux say 'the first wife of my husband' or 'the son of my husband's first wife' I took my head in my hands, certain that . . . this case represents nothing less than the trial of our lay society, of the rotten and immoral existence that the republic has inflicted on France."[17]

[15] Transcript, *Le Temps*, 24 July 1914.

[16] Paul Bourget, *L'Echo de Paris*, 27 July 1914. All quotes in this paragraph come from the *Echo* article except, "for something to be solid . . . institutions," which is cited in Georges Fonsegrive, *Mariage et union libre* (Paris, 1904), 29. Bourget's influence as a writer was enormous. Martin-Fugier called him "the author French women read the most"; see *La Bourgeoise*, 125.

[17] Guy de Cassagnac, *L'Autorité*, 22 July 1914.

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Madame Caillaux testifies before an all-male court. *L'Illustration*, 25 July 1914, © Sygma Photo News. Maitre Chenu, third from the left on the bottom row, is the Calmette family lawyer. The white-haired man toward the center is Maitre Labori, Mme Caillaux's attorney.

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Although right-wingers indulged in the most colorful rhetoric about divorce, journalists for the centrist, mass-circulation press made similar pronouncements. The pictorial weekly *La France illustrée* blamed Calmette's death on "the immorality of our decaying mores, the insanity of emotions that break all bonds and pile divorce on divorce."[18] The independent journal *Annales politiques et littéraires* explained that episodes like the Affaire Caillaux could occur in France because of the "evil ferments introduced into our mores by divorce."[19]

Although the trial of Henriette Caillaux took up the issue of divorce mainly in connection with Berthe Gueydan's testimony, press commentary kept the problem alive during the entire length of the proceedings. On the trial's last day, the politics of divorce returned to the courtroom itself when Charles Chenu made it part of his closing statement against Henriette Caillaux. The court, he suggested, owed Henriette not a hint of indulgence, for she was little more than a "mistress [who had] triumphed over the legitimate wife." Even less, he maintained, should it credit the testimony of Joseph Caillaux, who had "transgressed against the solemn oath he had given [Berthe Gueydan] by divorcing her in March, 1911."[20]

In the end, Chenu was unable to convict Henriette Caillaux of murder. Henriette had portrayed her act as a crime of passion, as a crime caused by feminine emotions beyond her conscious control, and this explanation, it seems, had swayed the court to her side. Parisian juries were notorious for excusing crimes of passion, especially those committed by women. In Henriette's case, sympathy for women of passion outweighed hostility to the moral toll of divorce.[21] Such was not the case for Joseph. Thanks in large part to Chenu's efforts, the political aura of this former prime minister later dissolved in the wake of questions about his morality and his private life. Inside the courtroom, divorce may have gone unpunished, but, outside it, the politics of divorce would dog Joseph Caillaux for years to come. The renewed vigor of French conservatism, with its emphasis on Catholic morality and the traditional family, had made Joseph Caillaux's private history of adultery and divorce central to France's public political life. The reasons for this lie at the very heart of France's attempt, after 1870, to create a fully secular republic.

MARITAL DISSOLUTION BECAME POSSIBLE in France only in 1884, when the new leaders of the Third Republic added divorce to the foundation of state laws and civil liberties designed to support the fledgling republican society. Like the new public education and the separation of church and state, the divorce law soon produced bitter and lasting conflict, conflict that revealed fundamental fissures in French society. So troubling was divorce that when the bill's main proponent, Alfred Naquet, first spoke on its behalf, the Assembly erupted in nervous laughter,

[18] *La France illustrée*, 21 March 1914.
[19] *Annales politiques et littéraires*, 29 March 1914.
[20] Transcript, *Le Figaro*, 29 July 1914.
[21] On the Parisian juries' reluctance to punish women for crimes of passion, see Ruth Harris, "Melodrama, Hysteria, and Feminine Crimes of Passion in the Belle Epoque," *History Workshop*, forthcoming. See also Joëlle Guillais, *La Chair de l'autre: Le Crime passionnel au XIX^e siècle* (Paris, 1986).

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refusing even to consider it.[22] Nor did its chances improve substantially after the abortive clerical coup of the *seize mai*, 1877. Republicans wanted no part of a bill that might alienate peasant electors. Still Naquet persisted, as did the leaders of France's nascent feminist movement, who had been crucial to this campaign from its inception.[23]

Inside Parliament, Naquet and his allies hitched themselves to the bandwagon of anticlericalism, maintaining that the family's laic liberation required divorce. In Naquet's words, the "spirit of clericalism" had stamped out the short-lived divorce law of the French Revolution, and now "the sweet revenge of the democratic spirit over the clerical spirit means the reestablishment of divorce."[24] Freedom from an unwanted marriage was as essential to the individual as all the other liberties promised by the secular state. Each French citizen, Naquet declared, must have the right to love, to feel passion, and to live life independent of the church's outmoded restraints.[25]

Attacked on all fronts, Catholic leaders responded to this issue as forcefully as to the Republic's efforts to secularize the schools. For clergymen, the assault on Catholic education and the revival of divorce were two sides of the same coin, as both threatened the stability of society itself. According to Mgr. Charles-Emile Freppel, a bishop who sat in Parliament, the Christian family was essential to France's very existence: "With all its vicissitudes and all its agitations, our country has had the sense to consecrate for itself one incomparable element of solidity, of permanence, and of cohesion: namely, the indissoluble family." Without it, France would cease to be a "Christian civilization," falling victim to what Freppel called a "semitic movement" (Naquet was Jewish) against its hallowed traditions. Freppel could not resist a *mot* in concluding that passage of Naquet's bill would "consummate the divorce between the Third Republic and the Catholic Church."[26]

Freppel did not ultimately succeed, but his efforts helped dilute the law that finally emerged from the Senate in 1884. In particular, divorce by mutual consent, central to the Revolutionary legislation of 1792, was rejected for a much more restricted bill. What would be called the Naquet Law, though its namesake was displeased on several counts, permitted divorce only on one or more of four specific grounds: adultery, cruelty, slander, and a serious criminal conviction.[27] To discourage divorce even more, the Senate prohibited marriage between the accomplices of adultery, and as a concession to the church, it denied Naquet's proposal to allow for a simple separation—Catholicism's lone solution to marital discord—to convert automatically into divorce on the request of husband or wife. The pro-divorce deputy Letellier expressed disappointment with the bill's final

[22] Alfred Naquet, *Le Divorce* (Paris, 1881), chap. 1.

[23] Leon Richter, one of the pioneers of modern French feminism, has been credited with drafting Naquet's original divorce proposal. See Claire Goldberg Moses, *French Feminism in the Nineteenth Century* (Albany, N.Y., 1984), 209.

[24] Naquet, *Le Divorce*, 14.

[25] Jacques Desforges, "La Loi Naquet," in Robert Prigent, ed., *Renouveau des idées sur la famille* (Paris, 1954), 107.

[26] Quoted in Desforges, "La Loi Naquet," 107–08.

[27] Wesley D. Camp, *Marriage and the Family in France since the Revolution* (New York, 1961), 71.

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form but justified it as "another crucial step towards the laicization of the state."[28] For committed republicans, the legalization of divorce, however truncated the measure, stood as a symbol of secularization on a par with Jules Ferry's reform of the schools.

Even though the law of 1884 satisfied no one group, its restrictiveness allayed the fears of some of its opponents and allowed the question of divorce to recede from view. But only temporarily, for, when organized feminism became more vocal in the 1890s, the issue of marriage and divorce resurfaced as part of a larger controversy over the changing role of women in modern society.[29] Most French feminists did not challenge their culture's reigning belief in separate spheres for men and women. Their efforts to open new possibilities for women—within the home and outside it—nonetheless provoked sharp responses from commentators of virtually all political hues. Feminism, even in its mildest version, aroused the most exaggerated, and contradictory, species of masculine fears. Women were deemed unequal to the world of manly professions and condemned at the same time for usurping men's jobs. They were praised for their maternal instincts but criticized for having too few children, exalted for their natural altruism while being chastised for their selfish pursuit of pleasure. And, most important to the issue of divorce, French women could be characterized as born for marriage yet denounced for taking it too lightly.[30]

This discussion raged throughout the Belle Epoque, but it assumed a sharper tone after the Dreyfusards' victory of 1899. Their success encouraged certain republicans and feminists to demand reforms in family law consistent with what seemed to be an ascendant individualism. In particular, these groups began to advocate a new concept of marriage. No longer was the institution to represent a sacred engagement for which husband and wife renounced their separate identities to create a new familial unity. Marriage was now to become, so argued its prospective reformers, a legal bond that joined two equal partners without depriving either of the distinct individuality they had brought to the union.[31] Republican men could champion this individualistic view of marriage as consistent with the era's emerging secular freedoms, while feminist women tended to support it as a way of making their gender's domestic and maternal roles freer, fairer, and

[28] Desforges, "La Loi Naquet," 108.

[29] For the early history of divorce in the Third Republic, see Henri Le Goasquen, *Le Divorce devant l'opinion, les chambres et les tribunaux* (Rennes, 1913). On feminism of the 1890s, see Karen Offen, "Depopulation"; Jean Rabaut, *Féministes à la Belle Epoque* (Paris, 1985); Claire Goldberg Moses, *French Feminism*; Steven C. Hause, *Women's Suffrage*.

[30] A characteristic, if lengthy, statement of such views can be found in Charles Turgeon, *Le Féminisme français*, 2 vols. (Paris, 1902). Turgeon was a well-known professor of law and political economy. See Emile Faguet's critique of him in *Le Féminisme* (Paris, 1910), chap. 1.

[31] The ferment over marriage in the Belle Epoque requires its own essay, and I will treat the issue in detail elsewhere. For the contemporary debate over what was then seen as the "crisis of marriage," see Georges Art, "La Crise du mariage," *Revue bleue*, 25 September 1897; Jean Joseph-Renaud, *La Faillite du mariage and L'Union future* (Paris, 1899); Paul Margueritte and Victor Margueritte, *Mariage et divorce* (Paris, 1900); "Enquête sur le mariage," *La Plume*, 15 April–15 June 1901; "Liberté du mariage," *La Revue socialiste*, September 1906; Leon Blum, *Du Mariage* (Paris, 1908); and Paul Lapié, *La Femme dans la famille* (Paris, 1908). In addition, many of the *enquêtes* on divorce, analyzed below, necessarily reveal contemporary attitudes about marriage as well.

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more fulfilling. As Karen Offen has made clear, activist women held no brief against marriage and the family, but their stance rested on the assumption that the evolution of both would endow the woman's separate sphere with her own individual voice.[32] Such "familial feminism"—Offen's apt term—was not, however, something the church and its allies on the right and center could accept. For them, the family retained its traditions or it was not a family at all.

France's debate over marriage and the family began to make headlines in the summer of 1900 when participants in the Second International Congress of Feminine Work and Institutions called for the liberalization of divorce. Their hope was to take divorce out of the hands of magistrates and allow the individuals involved to make their own decisions.[33] It did not take long for this and other such projects to catch journalism's roving eye. With divorce back in the headlines, the issue was ripe for politicization, especially after some advanced republican deputies introduced a bill to allow the dissolution of marriage by mutual consent.

By refocusing attention on divorce, these events revealed that virtually no group remained reconciled to the legislation of 1884. Republicans, particularly those most tinged with anticlericalism, found the law too tied to Catholicism's concept of marriage, one that deprived individuals of the freedom to end unhappy or oppressive unions.[34] Conservatives, both within the church and without, voiced an opposite view. For them, the divorce law, ill-conceived to begin with, had failed so miserably that the legislature should revoke it altogether. The law's proponents had promised fewer crimes of passion, less adultery, better marriages, higher birth rates, and fewer illegitimate children. But far from delivering these benefits, the conservatives claimed, divorce had simply made the old problems worse. According to Charles Turgeon, the antifeminist writer, "never before have the knife, acid, and the revolver served so frequently and so furiously the rancor of mismatched spouses."[35]

By the end of 1900, this controversy had erupted into a full-scale national debate, into a war of words conducted in the press through a promising new journalistic technique: the *enquête* or survey of public opinion. The first such survey was sponsored by the novelists Paul and Victor Margueritte, who, in 1900, sounded out elite opinion on marriage and divorce in successive issues of *La Revue*. Their effort was far from scientific, serving more to influence public opinion than to gauge it. Most of those polled wrote in favor not only of divorce by mutual consent but of divorce granted on the request of just one of the parties.[36] Shortly

[32] Offen, "Depopulation," 654.

[33] Marie Pégard, ed., *Deuxième Congrès international des oeuvres et institutions féminines . . .*, 4 vols. (Paris, 1902). See also *La Fronde's* coverage of this event in June, 1900.

[34] The brothers Paul and Victor Margueritte were the tireless champions of this position, and they helped break open the debate with their *Lettre ouverte* of 1 December 1900 to the legislature on the "imperfection of our current divorce law." See *Le Figaro*, 2 December 1900.

[35] Turgeon, *Le Féminisme*, 2: 241. For other similar statements by conservatives, see Le Goasquen, *Le Divorce*, 189; Morizot-Thibault, "La Femme et le divorce," *La Réforme sociale*, 42 (16 July 1901). Emile Faguet, who drew opposite conclusions, agreed with the conservatives' assessment of the divorce bill's failure. See his *Le Féminisme*, 196–97.

[36] Paul Margueritte and Victor Margueritte, "Mariage et divorce," *La Revue*, 35 (1 December 1900): 449–69; and 36 (1 March 1901): 485–506.

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thereafter, *La Revue* gave equal space to opponents of divorce, and, almost overnight, the sampling of opinion on marriage and divorce became an editorial vogue with a dozen major publications, highbrow and low, devoting space to it between 1901 and 1914.[37]

Paul and Victor Margueritte chose their respondents carefully. Most were Radicals, people whose commitment to personal freedom had drawn them first to Dreyfus and now to marital reform. In one typical contribution, the Radical deputy Charles Beauquier wrote that the state ought to have no interest in how long a marriage lasted, and he imagined a future in which unions would be based on contracts of limited duration, "three, six, or nine years, renewable like a lease." Marriage, for this parliamentarian, was a contract like any other, one into which the law could intervene "only to regularize and secure the situation of children."[38] Senator Auguste Delpech, also a Radical, went even further. Not only was marriage nothing more than a contract between free individuals but the state need not concern itself with the children couples produced. "To justify the restrictions built into our current [divorce] law, its defenders invoke the interest of society and of children; this is nothing but sophistry."[39] Another deputy, Paschal Grousset, expressed more concern for the fate of children. For him, "the legal aspects of marriage make sense only with respect to the child. Everything else about it that restrains individual liberty is a lie and an abominable tyranny."[40] More such libertarianism came from Lucien Leduc, a lawyer attached to the court of appeal: "Modern marriage . . . is nothing more than an ordinary contract through which each party hopes to develop in association with his complement."[41] Even the youthful Raymond Poincaré was swept up in the movement, advocating divorce by mutual consent but not "upon the request of one, though I have to admit that there are cases that would prove me wrong [in this hesitation]."[42] For Gustave Rivet, future editor of *Le Radical*, divorce was a "natural right" stipulated by Jean-Jacques Rousseau himself, who declared that "man must always be the master of his person."[43] Another respondent warned that if the divorce law did not change, people should not marry, for "as marriage is understood nowadays, it is a debasement."[44] Finally, Emile Zola was in favor of "the couple whose love makes their union indissoluble, but if divorce is necessary there should be no obstacles whatever."[45]

In a preface to their *enquête*, the Marguerittes joined their friend Rivet in explicitly associating the right to divorce with the philosophy and individualistic

[37] A few of these publications are cited in note 31; see also *La Revue socialiste*, *La Revue hebdomadaire*, *La Française*, and *Le Matin*.
[38] Margueritte and Margueritte, "Mariage et divorce," 485–86.
[39] Margueritte and Margueritte, "Mariage et divorce," 487–88.
[40] Margueritte and Margueritte, "Mariage et divorce," 489.
[41] Margueritte and Margueritte, "Mariage et divorce," 491.
[42] Margueritte and Margueritte, "Mariage et divorce," 494.
[43] Margueritte and Margueritte, "Mariage et divorce," 496. It is, of course, well known that Rousseau did not feel the same way about women, created, he believed, to serve men. See, for example, his novel *Emile*, trans. Barbara Foxley (New York, 1933), 324–24, 371–72.
[44] Margueritte and Margueritte, "Mariage et divorce," 496.
[45] Margueritte and Margueritte, "Mariage et divorce," 498.

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freedoms of the French Revolution. By secularizing marriage, "The Revolution, whose munificent breath of fresh air sent us toward a sun of justice and humanity . . . understood . . . that society and the law were made for the individual, and not the individual for society and the law. The role of both is none other than to safeguard individual liberty, not to destroy it through indissoluble engagements." For them, "Marriage was no more than a contract of fidelity, help, and mutual assistance, a free association between spouses."[46] They advocated divorce with arguments that have become familiar in our own time: it frees children from a home life filled with antagonism; it allows mismatched couples to be relieved of each other's company; and it forbids one spouse to chain the other to a union one party finds constraining or harmful. For good measure, the authors concluded with the tireless republican justification for everything: anticlericalism. Opposition to divorce reform was part and parcel of a "religious reaction" eternally hostile to "the principles of '92."[47]

At first glance, it seems surprising that these Radical republicans cast their arguments in the language of pure individualism. Radicals, after all, tended to question the liberalism associated with individualist positions, expressing allegiance in the 1890s to a new doctrine called solidarism. The proponents of solidarism intended it to correct the ills of classical liberalism while warding off the menace of collectivism. Specifically, solidarists proposed that the nation no longer be conceived of as an assortment of atomized individuals related to each other only through competition in the marketplace. It ought to be seen instead as a social body composed of interdependent organs, each performing a function necessary to the whole. Given this interdependence, no one could be an island of self-sufficiency, and all the nation's different members owed each other help and support.

Although this notion ruled out liberal individualism, it did not disown individualism itself. As Judith F. Stone has shrewdly pointed out, the ultimate purpose of solidarism was not to restrain private initiative but to reconstitute the individual sovereignty usurped by laissez-faire capitalism.[48] Solidarists argued that pure economic freedom had created such inequality in the marketplace that workers were now at the mercy of those who employed them. Private property increasingly escaped their grasp, as did the freedom it was believed to guarantee. By stressing cooperation, solidarists sought a rationale for government action designed to redress the balance of economic opportunity and personal freedom. But the ultimate aim remained the protection and enhancement of the individual. As the solidarist guru Léon Bourgeois wrote, the goal of Radical reform was a perfect meritocracy "in which every man will have reached, within the limits of justice, individual proprietorship."[49] This would be accomplished through progressive taxation, democratic education, and social insurance—reforms, the

[46] Margueritte and Margueritte, "Mariage et divorce," 453.
[47] Margueritte and Margueritte, "Mariage et divorce," 468.
[48] Judith F. Stone, *The Search for Social Peace: Reform Legislation in France, 1890–1914* (Albany, N.Y., 1985), 28–31.
[49] Bourgeois quoted in Theodore Zeldin, *France 1848–1945*, vol. 1, *Ambition, Love and Politics* (Oxford, 1973), 657.

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Radicals believed, that would free individuals from the tyranny of an unrestrained marketplace. These reforms would create a new kind of individualism, one consistent with the realities of modern industrialism.[50]

For Radicals, then, the end of a rejuvenated individualism justified the means of solidarist reform. But when it came to the family, no such elaborate reasoning was required. The problem there turned not on the existence of too much old-fashioned liberal individualism, as in the society at large, but on the lack of individualism of any sort. The family, Radicals believed—and socialists agreed with them—was a remnant of the Old Regime, with its hierarchic structure, its paternal authority, its Catholic culture of sacrament and ritual. As the writer Jean Joseph-Renaud expressed it, marriage and the family—unlike republican France's other institutions—suffered from "an unconscious residue of the Christian spirit, [from] the ingrained respect for its sacrament."[51] Likewise, one contributor to a survey in the literary review *La Plume* maintained that, "when it comes to marriage, the Revolution changed very little. To marry today, as in the past, is to sacrifice one's freedom."[52] As for individual family members, they "quest with energy, even violence," wrote the editors of the *Revue socialiste*, "for liberty, individuality and independence, renouncing the chains, rejecting the unnatural servitudes . . . the tyrannies reinforced by all-powerful traditions."[53] For these Radical and socialist writers, the family needed not a dose of solidarity but a renovation *à la* 1789. A family represented a tyranny that survived nowhere else.

The chorus of individualist comment on the French family is notable for its absence of female voices. It may be that, in considering marriage, republican men, far more than women, tended to value personal independence over familial solidarity. After all, pleas for the relaxation of marital obligations, for unrestricted divorce and marriage as pure contract, appeared to offer males considerably more than females. The remarriage market favored men, since divorce stigmatized women more. Marriage tended to be less crucial economically for men—particularly middle-class men—who had much greater earning power than their wives.[54] Beyond these reasons, it may be that, for some reformers, the rebellion against traditional marriage disguised a deeper male revolt against the responsibilities of wedlock itself or even a frustration against men's seeming inability to live without it.[55] In any case, the Marguerittes explicitly confirmed the masculine bias

[50] In addition to Stone's excellent discussion of solidarism, see Sanford Elwitt, *The Third Republic Defended: Bourgeois Reform in France, 1880–1914* (Baton Rouge, La., 1986), especially chap. 5; Zeldin, *France*, 1: chap. 21; J. E. S. Hayward, "The Official Philosophy of the French Third Republic: Léon Bourgeois and Solidarism," *International Review of Social History*, 6 (1961). For solidarist treatises from contemporaries, see Léon Bourgeois, *La Solidarité* (Paris, 1896); Celestin Bouglé, *Le Solidarisme* (Paris, 1907).

[51] Joseph-Renaud, *La Faillite du mariage*, 41.

[52] M. Saint-Pol-Roux, *La Plume*, 1 May 1901.

[53] "La Liberté du mariage," *La Revue socialiste*, September 1906.

[54] Martin-Fugier explains that divorce stigmatized women because men tended to consider them as "marked" or possessed forever by their first husbands. See *La Bourgeoise*, 95–99. For statistical evidence on the male advantage with respect to remarriage, see Camp, *Marriage and the Family*, 47–53. On women and work, see Louise A. Tilly and Joan W. Scott, *Women, Work, and the Family* (New York, 1978).

[55] The male marriage rate turned upward during the Belle Epoque, and, save for the war years, it subsequently remained well above the nineteenth-century average. See Camp, *Marriage and the Family*,

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of this individualist discourse on marriage and divorce. In response to objections that their proposed reforms would be harmful to women, they unabashedly declared, "Let's not suddenly become more feminist than the women!"[56]

There was little danger they would. Feminist and non-feminist women alike emphasized that marriage involved responsibilities—and solidarities—that made easy divorce less than ideal. In reply to the Margueritte' survey, the pioneering feminist writer Juliette Adam confided that growing marital instability gave her second thoughts about divorce in any form.[57] The novelist Marcelle Tinayre, herself a sincere feminist, told the Margueritte that she favored reforms only if the "material and moral welfare of children and women were taken into account."[58] Both women sensibly realized that, in a society such as theirs, unrestricted divorce could devastate women economically, especially middle-class women educated for marriage, not paid employment.[59]

Like Adam and Tinayre, other elite women tended to shy away from the Margueritte' more extreme proposals, as did leaders of the feminist mainstream, who wanted reform to go no further than divorce by mutual consent. Such caution toward divorce is significant in that women were more likely to request it than men. Women originated about 60 percent of all divorce proceedings, and they did so, it should be added, partly in response to a legal code that gave married women few rights of their own.[60] Nonetheless, feminist notables found no panacea in easy divorce. For them, improving marriage for women seemed more important than facilitating its dissolution. As Jane Misme, editor of *La Française*, remarked, "We will solve the problems of marriage, not by making it easier to leave our unions, but by increasing the possibility of staying . . . All the demands of feminism—namely, the same physical, moral, and intellectual education for both sexes; economic independence for women; equal marital rights for husbands and wives—help us toward this goal."[61] Feminists took particular umbrage at the idea

37. See also Barbara Ehrenreich, who argues for a more extensive masculine revolt in our own era, *The Hearts of Men: American Dreams and the Flight from Commitment* (Garden City, N.Y., 1983).

[56] "Ne devenez pas soudain plus féministes que les femmes!" Quoted in Henri Coulon and René de Chavagnes, *Le Mariage et le divorce de demain* (Paris, 1909), 135.

[57] Margueritte and Margueritte, "Mariage et divorce," 485.

[58] Margueritte and Margueritte, "Mariage et Divorce," 497.

[59] Martin-Fugier, *La Bourgeoise*, 10–14, 267–91.

[60] According to the Civil Code's infamous Article 213—the ultimate statement of the woman's enforced inferiority—the husband owed protection to his wife, the wife obedience to her husband. Elsewhere, the code maintained that the husband alone could choose where the family would live, and it gave him sole authority over the children his wife had borne. Wives needed their mates' permission to work outside the home, to enroll in the university, and until 1897 even to testify before France's civil courts. A husband controlled the family's finances, including his wife's salary, and, in a marriage governed by community property, he administered her dowry as well. Finally, the husband's adultery was "excusable," his wife's was not. On women and the civil code, see Jean Derruppé, "L'Evolution du droit français de la famille du début du siècle à la guerre de 1939," in Prigent, *Renouveau des idées sur la famille* 149–60; James F. McMillan, *Housewife or Harlot: The Place of Women in French Society, 1870–1940* (Brighton, 1981), 25–28. For statistics on the male-female ratio of requests for divorce, see Jacques Desforges, *Le Divorce en France: Etude démographique* (Paris, 1947), 39–40.

[61] *La Française*, 30 November 1912. In fact, feminists and their allies did succeed during the first decade of the twentieth century in enhancing women's marital rights. A pair of laws passed in 1907 gave married women the right to control their own earnings and to share authority over minor children equally with their husbands. In 1909, a new law obliged husbands to consult their wives before

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of dissolving a marriage at the request of one spouse alone. For Marguerite Durand, director of the feminist daily *La Fronde*, this radical version of divorce amounted to nothing less than the right for men to repudiate their wives.[62] If husbands could dissolve their marriages at will, women would be utterly subject to masculine caprice, to a man's primordial desire for youth and variety. In the words of Misme, divorce at the request of one spouse would become "the instrument for fantasies of the instinct."[63]

FEMINIST RESERVATIONS ABOUT DIVORCE failed to receive the attention they deserved; the male clamor for a more libertarian solution pushed them aside. But, as an avalanche of responses to the survey showed, extreme notions of reform provoked a reaction of fear and anger that sent the advocates of easy divorce scurrying for cover. By bringing the family question into the arena of public debate, the Margueritte and their allies opened a Pandora's box of emotional issues that conservatives could exploit for political advantage. For now, the right could use the new century's "crisis of marriage," its fears about depopulation, the emancipation of women, and the standing of men to shift the terms of political debate to its benefit. Having been defeated in the political realm, conservatives still possessed a cultural issue destined to renew their philosophy and help prepare the way for the "nationalist revival" to come.[64]

For the most part, participants—on the left as well as the right—in the debate set off by the Margueritte focused their attention directly on the family, on its problems and its essence. But their arguments were laden with strong political overtones. We have seen how progressives associated freedom and individualism in the family with anticlericalism and the ideals of 1789. Conservatives, for their part, connected the family's health and viability to that of the nation. If the family was in trouble, so was the nation; if it could be made whole again, so could France. They endeavored, in other words, to reduce all politics to the eternal metaphor

alienating family property, and, in 1912, women were granted the right to initiate paternity suits. See McMillan, *Housewife or Harlot*, 26; Moses, *French Feminism*, 229–30.

[62] Speech by Marguerite Durand, 28 February 1903, cited in Fonsegrive, *Mariage et union libre*, 183–84. Not all feminists agreed, of course, with leaders like Durand and Misme. Bradamante, a journalist who contributed to *La Fronde* supported any and all attempts to liberalize divorce, even those that might harm women in the short run. The Margueritte cite her comments in "Mariage et divorce," 501. Catholic feminists such as Marie Maugeret opposed divorce altogether, while certain Protestant members of the movement expressed serious reservations. On Maugeret and Catholic feminism, see Hause, *Women's Suffrage*, 64–67, 81–87; and Jean Rabaut, *Histoire des féminismes français* (Paris, 1978), 238–39. On Protestantism and feminism, see Hause, 37–39, 255–59.

[63] *La Française*, 30 November 1912. It should be noted that not only women expressed such reservations. The socialist historian Georges Renard feared excessive liberalization would "degenerate into the kind of freedom that enables men and women to change partners whenever their capriciousness moved them." René Viviani, still a socialist at this point, worried that divorce at the request of one party would give undue advantage to men, whose superior financial position and employability would allow them to remarry much more easily than women. These socialist views can be found in Margueritte and Margueritte, "Mariage et divorce," 495, 500.

[64] The standard work on the new nationalism of the pre-war years remains Eugen Weber, *The Nationalist Revival in France, 1905–1914* (Berkeley, Calif., 1968). See also Raoul Girardet, ed., *Le Nationalisme français, 1871–1914* (Paris, 1983), chap. 5; and Jean-Jacques Becker, *1914: Comment les Français sont entrés dans la guerre* (Paris, 1977), chap. 1.

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of family. The more they succeeded in doing so, the better positioned they were to attack the Radical republic, not just for policies believed to undermine the family but for their failure to structure society in the family's idealized image. In this context, divorce became central, for conservatives could accuse it of dissolving the institutions on which all others were based.

Again and again during these early years of the new century, publicists of the right identified the fate of the nation with the fate of the family. For the writer Maurice Maindron, "a nation is nothing other than a tightly unified family whose members must continually make sacrifices to the grandeur and the honor of its name." The abbé E. Carry defined the "patrie" as a concept "borrowed from the paternal household to designate an enlarged family." And F. A. Vuillermet, the author of books on youth and virility, extolled the family as the "essential foundation for the life of a nation and the necessary condition of its expansion."[65]

In general, conservatives claimed that whatever menaced the family menaced fatherland and state as well. Writing in *Le Figaro*, Arvède Barine (a.k.a. Mme Cecile Vincens) declared that "divorce is at this very moment working to bring about the fall of the Third Republic." An anonymous contribution to *Fémina*, a popular women's magazine, blamed divorce for "destroying family and with it the fatherland itself." Antifeminist author Charles Turgeon summed up all the worst fears of the conservatives about the relationship between *famille* and *patrie*. "Revolutionaries and feminists," Turgeon wrote, "now understand that the way to overturn our social order is not to bring down the government. As soon as one falls, another rises in its place." Our revolutionaries realize, he continued, that the real target, "the grand enemy that must at all costs be undermined and then destroyed" is the family, "that ancient domestic power which stands as the last refuge of social authority."[66]

For conservatives, paternal authority constituted one of the most pivotal issues. Because divorce freed a woman from her husband's tutelage and awarded custody of children to mothers, it led, argued the legal scholar Jacques Peltier, to the decline of paternal authority.[67] This development worried Turgeon and others who feared for the persistence of authority itself. "We are losing the sense of authority," he lamented. "So enamored have we become of equality and independence that we can hardly endure—let alone honor—*le patronat*, the leadership or even the preeminence of fatherhood." Nothing was more regrettable for Turgeon than "the weakening of authority in the family."[68] By sapping authority at its very source, divorce and the ills that accompanied it drained fathers, rich and poor, of the strength to command.

Beyond these moral and ideological admonitions, conservatives responded to the call for easier divorce with some alarming statistics. The annual number of

[65] Maurice Maindron in *La Plume*, 15 April 1901; E. Carry, *Famille et divorce* (Paris, 1908); F. A. Vuillermet, *Le Suicide d'une race* (Paris, 1911).

[66] Arvède Barine, *Le Figaro*, 2 December 1902, cited in Fonsegrive, *Mariage et union libre*, 169; *Fémina*, 15 October 1904; Turgeon, *Le Féminisme*, 2: 466.

[67] Jacques Peltier, *Le Divorce et la puissance paternelle* (Paris, 1908).

[68] Turgeon, *Le Féminisme*, 2: 366.

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divorces, modest in the new law's early years, was beginning to rise sharply. The courts had dissolved some 5,000 marriages in 1887 and 10,000 in 1903, growing to nearly 17,000 in 1912. Even more troubling than actual divorces was the annual number of requests for divorce: 4,600 in 1885, 19,000 in 1912—more than a four-fold increase in less than three decades. The number of marriages per year stayed about the same during this period, producing by 1913 a ratio of about one divorce for every eighteen marriages. This figure, low by our own standards, was worrisome for many in a country new to divorce, especially for Parisians, whose rate was double that of the country as a whole: one divorce for every nine marriages.[69]

By themselves, the mounting divorce statistics disturbed conservatives enough; viewed side by side with the plummeting birth rate, they created a furor. Between 1880 and 1901, the number of live births per 10,000 inhabitants declined by nearly 14 percent from 250 to 216 and would drop another 13 percent by 1911. France was soon to have the smallest population of any major European power and soon to be half the size of Germany.[70] Most of those who wrote on depopulation after 1900 blamed it in part on divorce.[71] A short marriage, they argued, logically produced fewer children than one lasting a lifetime.[72] And the possibility a marriage might not last encouraged young couples to limit the number of children they bore. Divorce, conservatives added, was yet another manifestation of the pernicious individualism that made French men and women unwilling to sacrifice personal pleasure and material comfort to repopulate the nation.

When people on the right began to insert their own *enquêtes* into the French press, they used the occasion not just to oppose divorce reform but to challenge the right of divorce itself. The director of a prominent survey in 1903, Frédéric de France, objected to liberalized divorce on grounds of the male's instinctual quest for variety. Men, he wrote, have "a kind of instinct for conquest, a latent desire for caprice." And, for that reason, the idea that men could end a marriage whenever they wanted "frightens me like a horse without reins."[73] Other respondents took a similarly dim view of liberalized divorce. According to economist Anatole Leroy-Beaulieu, the Marquerittes' reforms would lead to a "serial polygamy" that would "sacrifice the essential interests of society and the family not, as some say, to the individual's interest, but to his whims and his passions." The writer Jules Bois was convinced that women would suffer much more than men from divorce on demand, and Madame Alphonse Daudet, echoing many of de France's respondents, believed divorce in any form "martyrs and

[69] Charles Turgeon presented many of these figures in his opus, *Le Féminisme*, especially 2: 240–42. More complete numbers can be found in Desforges, *Le Divorce*, chaps. 1–4, especially p. 54. See also Camp, *Marriage and the Family*, 80.

[70] Desforges, *Le Divorce*, 172–73; Hause, *Women's Suffrage*, 22; and Hunter, "The Problem of the French Birth Rate," 491.

[71] See, for example, Louis Delzons, *La Famille française* (Paris, 1913); Vuillermet, *Le Suicide d'une race*; and Henry Clément, *La Dépopulation en France* (Paris, 1910).

[72] Conservatives produced some dramatic statistics to support this claim. In 1900, 44 percent of all divorced people had no children. See Fonsegrive, *Mariage et union libre*, 219.

[73] Frédéric de France, "La Question du divorce," *La Revue*, 44 (1 March 1903): 528–39.

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demoralizes our children." Finally, several writers argued that the campaign for divorce was, in reality, a campaign for the *union libre*, the free union whose transitory coupling amounted to little more than free love. As the sociologist Gabriel Tarde explained, "as soon as we begin down the road to divorce, logic necessarily commands us to go all the way, all the way to mutual repudiation for no particular reason. If we seek to judge and reform marriage from the individualistic stance of providing the greatest happiness for the spouses, then our ideal, whether we realize it or not, is the free union."[74]

Emile Durkheim, writing in *La Revue bleue*, echoed his fellow sociologist's reservations about the campaign for liberalized divorce.[75] Durkheim apologized for appearing to sound a reactionary note but went on to oppose divorce by mutual consent and even to question the desirability of divorce in any form. In his earlier study of suicide, Durkheim claimed to have discovered a strong relationship between divorce and the propensity for individuals to take their own lives. Other commentators had concluded from similar findings that divorce caused suicide and that divorced people were much more likely to kill themselves than were married ones. But Durkheim saw things as more complicated and more ominous. By looking closely at the data for regions with high divorce rates, he found that the increase in suicides came not from divorced people but from married ones. Paris, in other words, exhibited a higher suicide rate than the provinces because far more married people in the capital killed themselves than did those outside it. Single people in the city, by contrast, were only slightly more likely to kill themselves than were single people in the country. He explained this observation by suggesting that divorce was dangerous not simply in its effect on individuals but also in its effect on marriage as an institution. The purpose of marriage, Durkheim maintained, was to promote order and security, stability and instinctual restraint. When it did so, marriage "creates for both spouses a moral constitution that is sui generis," one that shields them from suicide. What made divorce so pernicious, he wrote, was its ability to weaken *all* marriages whether they broke up or not. The possibility of divorce encouraged partners to envision a new and better marriage to replace the existing one, judged always in relation to an imaginary marital ideal. These thoughts engendered a longing for novelty and variation that rendered happiness in marriage all but impossible. In extreme cases, the misery that resulted could lead to suicide.[76]

To this argument, Durkheim appended a revealing discussion of differences between the sexes. When he classified his statistics by gender, he discovered that the relationship between high rates of divorce and suicide held true only for married men, not married women. In comparing married and single men, Durkheim noticed that the former killed themselves two to three times more often in regions of high than low divorce, while single men were no more likely to commit

[74] de France, "La Question du divorce," 528–39.

[75] Emile Durkheim, "Le Divorce par consentement mutuel," *La Revue bleue*, 5(5May 1906): 549–54. The argument Durkheim presented here echoed the one he had elaborated earlier in his influential work, *Suicide*.

[76] Durkheim, "Le Divorce," 550–52.

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suicide in high regions than low ones. The prevalence of divorce thus affected married men in particular, multiplying their suicide rate as much as three times. For women, the effect of a high rate of divorce was altogether different. Living in Paris (high divorce) as opposed to the provinces (low divorce) did not raise suicide rates for married women more than for single ones. Thus the availability of divorce hurt married men but did not raise the number of suicides for women. From this, the sociologist asserted that secure marriage protects men from suicide but not women. He refused in this *Revue bleue* article to go so far as to repeat his earlier contention in *Suicide* that "the effect of divorce was to slightly increase wives' immunity [to suicide]."[77] But his article left the impression nonetheless that marital stability shields men from suicide while divorce shields women. This conclusion, of course, is not the one he intended to make, since his purpose was to show that divorce was bad in general. But some readers, especially feminist ones, might have deduced from Durkheim's article that divorce could be a good thing for women.

Why, in Durkheim's view, did divorce figure significantly in men's suicide rates while it affected women's so little? His answer rested on the commonly held belief that men alone possessed restless instincts that had to be tamed. Unlike women, whose "moral constitution" remained unchanged by marriage, men needed the institution to produce a set of rules "to which the passions must submit." By directing masculine desire to a single object, "certain, defined, and in principle invariable, [marriage] prevents the heart from becoming agitated, from tormenting itself in a futile quest for goals always new and always changing." Marriage alone makes possible "the emotional peace, the inner equilibrium, that comprise the essential conditions of happiness and of healthy morals." But marriage worked this way, Durkheim was quick to add, only when its rules enjoyed universal respect. "To the extent that the bonds of marriage become fragile, or that they can be broken at will, marriage ceases to be itself and thus to yield the same virtues." Divorce saps marriage of its force and subverts the rules men need to contain their instincts, instincts that promise pleasure and produce only pain. This is why, Durkheim wrote, "the proposed liberalization of divorce will lead inevitably to higher rates of suicide." The measure purports "to alleviate the moral misery of husband and wife, but its result will be to demoralize them and to detach them all the more from life itself."[78]

This conclusion overlooked the distinctions he had just made between men and women, distinctions that implied—contrary to received opinion—that marriage benefited men more than women. That Durkheim stopped short of saying so directly suggests an uneasiness about such a judgment. Men commonly believed that marriage compromised their freedom and that, by agreeing to it, they made a sacrifice for which women owed them eternal gratitude. The notion, therefore, that men needed marriage more than women threatened one of the main assumptions on which the "strong sex" based its power or, at the very least, its sense of superiority.

[77] Durkheim, "Le Divorce," 551.
[78] Durkheim, "Le Divorce," 552–53.

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IT WAS ONE THING FOR HIGH-BROW PUBLICATIONS like *La Revue* and the *Revue bleue* to run such articles as Durkheim's, quite another for a large-circulation women's magazine like *Fémina*.[79] Beginning in September, 1904, and continuing for three months, *Fémina* conducted an *enquête* that took the unusual tack of inviting ordinary readers to write in. Some 3,000 letters were received, a narrow majority of which sided with Paul Bourget's belief in indissoluble marriage over the Margueritte' quest for divorce on demand.[80]

Fémina's survey opened with a debate between Bourget and the brothers Margueritte, who each had recently published a *roman à thèse* to dramatize their views on divorce. The Margueritte' novel, *Les Deux vies* (1902), told the sad story of a virtuous young woman married to a cad who refused to grant her a divorce. The judge ruled in the husband's favor, giving the wife no choice but to leave home penniless and without her children. The moral hardly needed to be stated: just and fair divorce laws were required to free women from cruel and all-powerful husbands. Bourget's novel, *Un Divorce* (1904), took the opposite stance. The heroine is a divorced woman whose remarriage brings her nothing but grief. Even though the first husband had been hideous and the second loving, her actions estranged her from her children, from the church she valued, and eventually from the second husband she once admired so much. The moral: divorce always caused more problems than it solved, with disastrous consequences to individuals as well as families.[81]

Both novels created a huge stir in the early years of the new century, and it is significant not only that thousands of readers wrote in but that Bourget's uncompromising position against divorce received the majority's support. In part, the results reflected a readership of middle-class women dependent on marriage for economic reasons. But they also mirrored a middle-class caution on matters of morality and family. Those who opposed divorce in *Fémina*'s survey commonly did so on patriotic grounds, viewing the family as basic to the existence of the *patrie*. In the words of one writer, the "fatherland is none other than *le plus grand foyer*."[82] By implication, then, when the *famille* was in trouble, so was the *patrie*. Or, as the conservative reformer Emile Cheysson put it elsewhere, "Tant vaut la famille, tant vaut la nation."[83]

By the autumn of 1904, when *Fémina*'s survey appeared, France's debate over divorce had entered its fifth year. The nation seemed almost obsessed with a

[79] In his 1913 law thesis, *Le Divorce devant l'opinion* (Rennes, 1913), Henri Le Goasquen commented, "That a women's magazine largely devoted to fashion has raised the question of divorce, confirms, it seems to us, the depth and persistence of the nation's interest in this problem," 209.

[80] Bourget's position won by fifty-two votes in a field of 3,000 responses. See Paul Abram, *L'Evolution du mariage* (Paris, 1908), 140.

[81] The central characters in works by writers like Bourget and the Margueritte often tended to be women, no doubt to appeal to their mostly female readership. These "social issue" novels written for women deserve much more study than they have received, but, for a brilliant example of what can be done, see Martin-Fugier, *La Bourgeoise*, especially parts 2 and 4. Bourget turned *Un Divorce* into a play four years later and produced it with considerable success.

[82] *Fémina*, 15 October 1904.

[83] "The worth of the family is the worth of the nation." Quoted in *La Réforme sociale*, 1 April 1901: 404. On Cheysson, a key figure in the Belle Epoque's debate over family and society, see Elwitt, *The Third Republic Defended*, chap. 2.

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problem that had captured perhaps more attention from ordinary citizens than the parallel struggle among religious and political leaders over the separation of church and state. It may be that conservatives outside the church found it more fruitful to attack what they took to be republican mores than to defend the church and the religious orders directly. Or it may be that the health and viability of families, and of the nation as the consummate family, were matters that directly concerned the vast majority of French men and women. But, whatever the reasons, the debate on divorce raged on. Novels devoted to the issue rolled off the presses, as did doctoral dissertations, psychological studies, pieces of theater, journalistic diatribes, and above all the *enquêtes*.[84] It was almost as if the French press, increasingly alert to ways of shaping public opinion, wanted readers to respond directly, if only to gauge journalism's success. After *Fémina*, similar surveys appeared in *Gil Blas*, the *Revue socialiste*, *Revue hebdomadaire*, *La Française*, and *Le Matin*, to name a few.[85] The survey in *Le Matin* was especially important because of the paper's enormous daily circulation (about 650,000) and because numerous readers responded.[86] *Le Matin* conducted its survey on "The Crisis of Divorce" from early January to late February of 1908, amassing more than 6,000 responses.[87] Virtually every day, the front page featured excerpts of letters from ordinary readers, and the survey proved so popular that beginning in March the paper devoted a feuilleton to the subject. This was an issue that knew no bounds.

On 8 February 1908, for example, survey responses filled the three middle columns of *Le Matin*'s front page, displacing news of a coup d'état in Portugal as well as the armed struggle in Morocco. International affairs gave way to the most domestic of affairs, and *Le Matin*'s editors exploited an insatiable interest in divorce with banner headlines: "Divorce: Confession générale d'une époque" (9 February 1908); "Les douleurs du divorce" (12 February); "La Crise du mariage et du divorce est portée devant le Sénat" (22 February). The paper made a token effort to give equal space to all sides of the issue, with one column reserved for advocates of divorce, one for opponents, and one for partisans of the *union libre*. But, since the latter term implied divorce on demand, survey director Hughes Le Roux ensured that his own individualist views would enjoy the most attention.

Little in the content of the letters was new. Readers condemned divorce for destroying *famille* and *patrie*, and they supported it for relieving the pain of familial strife. Conservatives vilified the *union libre*, while individualists sang its praises. But, despite the stale content, journalists found something novel in its form. Many of

[84] In addition to the *enquêtes* already cited, see the law theses by Alfred Valensi, *L'Application de la loi du divorce en France* (Montpellier, 1905), and by Henri Rousseau, *La Vulgarisation du divorce* (Poitiers, 1903); the religious tirade by Abbé Paul Bellée, *Du divorce au mariage* (Paris, 1913). In addition, the era witnessed the pro-divorce plays of Paul Hervieu, "Les Tenailles" and "La Loi de l'homme"; the pro-family sociology of *La Réforme sociale*; and countless works of journalism, such as Hughes Le Roux's *Le Bilan du divorce* (Paris, 1909), and Henri Coulon and René de Chavagnes's *Le Mariage et le divorce de demain*.

[85] *Gil Blas*, December 1905; *La Revue socialiste*, September 1906; *Le Matin*, January–February 1908; *La Française*, January 1910; *Revue hebdomadaire*, January 1909.

[86] Statistics for the circulation of Paris newspapers in 1910 and 1912 can be found in Claude Bellanger, *et al.*, *Histoire générale de la presse française*, 4 vols. (Paris, 1972), 3: 296.

[87] *Le Matin* reported this figure in its issue of 18 February 1908.

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the letters contained narratives of personal trauma and individual redemption, narratives that offered the same mix of titillation and moralism that made the *fait divers*, or crime brief, so popular with newspaper readers. By printing such personal confessions, the editors of *Le Matin* could assimilate a key issue of public debate to the *fait divers* without sacrificing their aura of journalistic respectability. They could, in other words, promote domestic dramas to the front page by disguising them as philosophical argument. It was a technique that enabled *Le Matin* to fuse public and private in a reportage whose guiding principle was the *fait divers*.[88]

The letters themselves could almost have been plot outlines for the era's innumerable social novels. One writer recounted how he had sacrificed the loving *union libre* of his student days for a bourgeois marriage that would soon end in divorce. The prose was simple and direct: "I knew the happiness of a *union libre* in my student life. I got married. And now I am in the process of divorce." The writer confessed that a mindless attachment to "bourgeois respectability" had ruined his life and destroyed his love. "At the age of twenty-six, I left my mistress whom I lived with quite happily. These old [bourgeois] ideas had gotten the better of me, and I married a woman with a mentality entirely different from my own. And now to free myself, I am condemned to expose all my conjugal misery before a judge . . . I am, therefore, a partisan of the *union libre*."[89]

Another letter came from the former wife of a cabinet minister who had used his power and connections to gain custody of their child. The minister, she wrote, had made it almost impossible to visit her daughter, having over the years turned the child against her. Here was a purely personal narrative, one that argued neither for nor against divorce but simply recounted its ravages.[90] Similarly, a third excerpt presented a "lettre d'un enfant" written by a boy who had been nine years old when his parents separated. After an extended tug-of-war for the child, the father simply seized custody, thanks—the boy wrote—to the police. Shortly thereafter, his father forced him to compose hateful letters to his mother accusing her of every sort of heinous crime against parenthood. The letters ensured a court judgment against his mother and permanent custody for his father. The mother's failure to appear at the divorce trial convinced the boy that she had died and that his letters had caused her death. There followed a list of more horrors by the father and always the nagging guilt of the child: "I think about only one thing, what they made me do to my mother. My beloved mother. They took me away from her, and I have never stopped crying for her." The letter ended with a threat, one that promised to turn a small *fait divers* into one of larger proportions. "When I'm all grown up, if I find out that my mother died because of the letter they made me write, I will be a great criminal, because I will have my revenge. I will avenge my

[88] On the *fait divers*, see Michelle Perrot, "Fait divers et histoire au XIX[e] siècle," *Annales: Economies, sociétés, civilisations*, 38 (July–August 1983): 911–19; Roland Barthes, "La Structure du fait divers," in *Essais critiques* (Paris, 1964): 188–98; Romi, *Histoire des faits divers* (Milan, 1962); Musée national des arts et traditions populaires, *Le Fait divers* (exhibition catalogue) (Paris, 1982).

[89] *Le Matin*, 9 February 1908.

[90] *Le Matin*, 10 February 1908.

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mother; I will kill my father."[91] In this letter, like the last, there remains no hint of political exchange, only a tale of domestic woe laced with oedipal fantasies jolted to consciousness.

After two months of relentless publicity, *Le Matin* closed its survey with a flair of journalistic sensation. The letters, wrote Le Roux, revealed "all the ravages of our ravaged era, an era in which paternal authority weakens from one day to the next, the conjugal bond dissolves almost to nothing. We have seen the birth of women's rights, at the cost of a thousand dramas, and we have watched the family fade away, with our children split up between mothers and fathers."[92] The journalist may have meant this as the final word, as the moral summation of his times, but, for almost everyone else, it represented just one more link in a chain of collective fixation on divorce.

It was a chain that remained unbroken in the years before 1914 as conservatives and reformers rehearsed the debate over divorce again and again. The closer France came to war, however, the more the conservatives seemed destined to win. Those who responded to a survey in the feminist daily *La Française* in 1910 expressed serious reservations about divorce, even though women still requested it more often than did men. A Catholic congress held in Bordeaux in 1912 evoked considerable sympathy in the press for the traditionalist position. Two years later, a survey of well-educated women ages 18–25 indicated a general agreement that divorce "painfully mutilated a woman's life" and that people should resort to it only in extreme cases, especially when children were involved.[93]

WITH ALL THIS FERMENT OVER MARRIAGE, DIVORCE, and the family in the waning years of the Belle Epoque, it is no wonder that moral issues figured prominently in the trial of Henriette Caillaux. Conservatives had linked the health of *famille* to the strength of *patrie* and in doing so had encouraged French men and women to see themselves as members of a national family rather than as equal citizens of an egalitarian republic. The new concept had resonated widely in the years before 1914, permitting conservatives to attack the Caillauxs for crimes against the family and against the nation forged in its image. Challenges to Joseph Caillaux's record in government had failed to topple him in a country in which party affiliations changed little. But the shifts registered at the level of culture and morality had made Joseph Caillaux more vulnerable. By judging him against these stricter standards, conservatives finally achieved what parliamentary and electoral tactics could not. Despite the trial's favorable verdict—an outcome Henriette owed to her claim of feminine frailty and irresponsibility—the case effectively condemned Joseph to political purgatory for more than a decade.[94]

[91] *Le Matin*, 18 February 1908.

[92] *Le Matin*, 22 February 1908. La Roux seems to have abandoned the individualist perspective that had shaped the survey itself. It should be noted, in addition, that *Le Matin*, surprisingly progressive on some issues, stood fast against demands for women's suffrage.

[93] Amélie Gayraud, *Les Jeunes filles d'aujourd'hui* (Paris, 1914), 48–50.

[94] He would make a remarkable comeback in 1925 with his appointment as the minister of finances. Joseph Caillaux finished out his political life in the Senate, where he was influential enough to help

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Before the trial, President Poincaré had reportedly confided to Jean Dupuy, editor of *Le Petit parisien*, that he would have no choice but to appoint Joseph Caillaux prime minister "if his wife is acquitted."[95] But, after the trial, acquittal and all, such an appointment was out of the question. The affair's damaging portrayal of Joseph Caillaux's moral life, followed by the outbreak of World War I, served not only to keep him out of government but began a process that would culminate in his own imprisonment four years later. The charges accused him of acts bordering on treason, on crimes against the extended family of wartime France. And, though charged unjustly, Joseph Caillaux was finally compelled to suffer for the moral liberties he had taken, moral liberties that, in the minds of conservatives, had made him a traitor to the family and to the nation it preserved.

bring down Leon Blum's Popular Front government in 1937. On Joseph Caillaux's career in the 1920s and 1930s, see Jean-Claude Allain, *Joseph Caillaux*, vol. 2, *L'Oracle, 1914–1944* (Paris, 1981), books 5 and 6.

[95] Joseph Caillaux, *Mes Mémoires*, vol. 3, *Clairvoyance et force d'âme dans les épreuves, 1912–1930* (Paris, 1947), 152. It is true that this information comes from Joseph himself, but historians have tended to believe in its accuracy because Poincaré never contradicted it. See Brédin, *Joseph Caillaux*, 137.

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The Debates over the Asiatic Mode of Production in Soviet Russia, China, and Japan

JOSHUA A. FOGEL

A GREAT DEAL HAS BEEN WRITTEN about the "Asiatic mode of production," most of it the work of Marxists around the world, as they tried to establish what Karl Marx and Friedrich Engels meant by the expression and what it signified for the contemporary "Orient." Over the past decade, several scholars less involved in the Marxist discussions have addressed the debates that emerged over the Asiatic mode of production as topics in the intellectual history of Russia, China, and Japan of the 1920s and 1930s.[1] More recently, a flurry of some thirty or more articles appeared in the People's Republic of China on the issue, but the topic disappeared from scholarly journals as quickly and mysteriously as it had arisen, only to be revived again in 1985 and 1986. None of the writings on the Asiatic mode of production has yet attempted an examination of the issues raised in all the major countries in which there was debate—primarily, the Soviet Union, China, and Japan—and of the important interrelationships between them from the late 1920s through the late 1930s. Without appreciating the transnational context in which the debate took place at that time, we cannot fully understand it.

The idea of a dynamic, progressing, participatory polity in the West as opposed to a static, unchanging, despotic polity in the East goes back to the self-perception of the Greeks vis-à-vis the Persians. When Aristotle's *Politics* was translated into Latin in the thirteenth century, its notion of Oriental despotism was reintroduced into European intellectual currency. Probably in the fourteenth century, a lack of the right to private property was added to this concept. It was Montesquieu who reunited "despotism" with the Orient in his attacks on French absolutism. China was then pictured as a stable, perfectly rational, enlightened despotic state.[2]

This portrayal of China did not change as European conceptions of the state did. As progress or democracy became elemental to the "modern" nation-state, China (and Asia generally) was found woefully wanting. Asian stagnation, backwardness,

[1] Marian Sawer, *Marxism and the Question of the Asiatic Mode of Production* (The Hague, 1977); Stephen Dunn, *The Fall and Rise of the Asiatic Mode of Production* (London, 1982); Arif Dirlik, *Revolution and History: The Origins of Marxist Historiography in China 1919–1937* (Berkeley, Calif., 1978); and Germaine A. Hoston, *Marxism and the Crisis of Development in Prewar Japan* (Princeton, N.J., 1986), especially chapter 6.

[2] Basil Guy, *The French Image of China before and after Voltaire*, vol. 21 of *Studies on Voltaire and the Eighteenth Century* (Geneva, 1963); E. Rose, "China as a Symbol of Reaction in Germany, 1830–1880," *Comparative Literature*, 3 (1951): 57–76; and Lawrence Krader, *The Asiatic Mode of Production: Sources, Development and Critique in the Writings of Karl Marx* (Assen, 1975), 62–67.

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and despotism were seen as the diametrical opposites of Europe's own self-congratulatory image as modern, developing, progressive, and free. For Georg W. F. Hegel, by way of example, China and India lay "outside the world's history"; for Leopold von Ranke, the Chinese were one of the "races of eternal standstill"; and, for Johann Herder, "in China and India, . . . there is no true historical progress but only a static unchanging civilization."[3] James Mill and John Stuart Mill regarded the Orient as fixed to its unchanging village and unable to join the course of history without a jolt from outside. In none of these cases did this blanket portrayal of Asia or China emerge from research or even personal travel and observation. "Asia" (and its adjectival form, "Asiatic") had become terms of derogation.

Despite Marx's precocious vision of the Taiping Rebellion, he shared the image of China as socially stagnant. He characterized "Asiatic" or "Oriental" society by the "*absence of private property* in land," the presence of large-scale irrigation works, an omnipotent state ruling in a uniformly despotic manner, and communal landownership: a complete lack of social dynamism.[4] Because the state monopolized all economic initiative in the Orient, Marx argued, Oriental society remained stagnant, unable to rechannel surplus profit or to produce class differentiation, class consciousness, and finally class struggle, the essence of all history in the words of the *Communist Manifesto*.[5]

Engels, through his works *Anti-Dühring* and *The Origin of the Family, Private Property, and the State*, published after Marx's death, attempted to integrate Marx's ideas on Oriental despotism and Asiatic society into the two men's larger theoretical system. For example, in *Anti-Dühring*, Engels seemed to be saying that the ruling class in the Orient was a political-administrative elite, while in the West, it was an economic class. He also emphasized the isolated, segmented village nature of Oriental society, saying little or nothing about large irrigation works.[6]

As the debates unfolded in the twentieth century, no one definition of an Asiatic mode of production emerged. Certain qualities on which most debaters tacitly agreed, however, can be identified. The Asiatic mode was characteristic of a society in the "East" ruled by an all-powerful despot who held final control over all matters secular and religious: Oriental despotism. This society was agrarian and highly dependent on massive irrigation projects that any given locality was unable to provide for itself; thus, every local community was dependent for its lifeblood on the center that provided these hydraulic works. Private property was not recognized, inasmuch as the despot owned all the land. And the village was the primary unit of society. Thus, the social order based on the Asiatic mode of production lacked so much as the suggestion of dynamism.

[3] As cited in Joshua A. Fogel, *Politics and Sinology: The Case of Naitō Konan (1866–1934)* (Cambridge, Mass., 1984), 3–5.

[4] Karl Marx to Friedrich Engels, 2 June 1853, Karl Marx and Friedrich Engels, *Selected Correspondence 1846–1895*, Dona Torr, trans. (1942; rpt. edn., Westport, Conn., 1975), 64–66.

[5] In a similar vein, George Lichtheim noted that "the Orient had not evolved anything corresponding to private property in land: unquestionably one of the preconditions of feudalism"; *Marxism: An Historical and Critical Study* (New York, 1961), 158.

[6] Sawer, *Marxism and the Question*, 43–52, 69–71, 75.

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The Asiatic mode of production seemed to explain to Marxist synthesizers much that failed to fit Marxist theory properly. It enabled them to bring China, India, and other enormous areas of the world (probably the majority of the earth's population) into their world-historical conceptions. China certainly had experienced many centuries of despotism; waterworks were extremely important; and, to an untrained eye, ancient China and contemporary China looked remarkably alike. But such a blanket explanation tends to obfuscate much more than it clarifies.

As scholarship on China and Japan improved over the years, simplistic notions such as the Asiatic mode of production no longer attracted the kind of attention they once did, even among Marxists. Nonetheless, in looking back over the Marxist debates in Russia, China, and Japan over the Asiatic mode of production, we must take the contributors and their ideas seriously but not exclusively at face value. Understanding requires appreciation not only of the literal content of the discourse in these three countries but the intellectual, political, and other motivations as well; such motivations do not vitiate the importance of the discourse itself any more than the surface impression of these debates can satisfy scholars.

MARX'S BRIEF MENTION OF THE ASIATIC MODE OF PRODUCTION as one of four societal formations in his preface to *A Contribution to the Critique of Political Economy* (hereafter, *Critique*) is well known: "In broad outline, the Asiatic, ancient, feudal and modern bourgeois modes of production may be designated as epochs marking progress in the economic development of society." Until the 1940s, that one sentence and scattered references in *Capital* and elsewhere constituted the strongest argument within the Marxist camp for the existence of an Asiatic mode of production. It was Soviet scholars who opened the debate, even if, on the surface, the Asiatic mode of production would seem to be of little importance to them. There was, of course, the prevalent view of prerevolutionary Russia (held by Marx among many others) as an extremely backward, indeed reactionary, society. Yet this same society had produced the first socialist revolution.

A debate centrally concerned with the Asiatic mode of production erupted in the mid-1920s in the Soviet Union, at precisely the same time as the battles in the Comintern over the correct course for the Chinese revolution to pursue. The two events were no coincidence, to be sure, as frankly admitted at the time by participants in the controversy. If Chinese society could be characterized as feudal or semi-feudal, then a "bourgeois-democratic revolution" was the order of the day, and an alliance with the "bourgeois" Kuomintang, as demanded by the Comintern, was appropriate. If, however, China could be described as "Asiatic," that meant China had a weak, underdeveloped bourgeoisie on which the revolutionary leadership could not safely rely; the peasants and the proletariat had to lead the revolution, and it could be socialist in aim.

Evgenii S. Varga, a Hungarian who had come to Russia after the failure of the 1919 Revolution in his native land, opened the debate in 1925 with an essay denying that China was feudal. Admitting the influence of Max Weber (because of Weber's emphasis on China's literati, the country's incapacity to develop a

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dynamic bourgeoisie, and the nature of Chinese social organization), Varga claimed that China's ruling class was a scholar-elite, that the "tyranny of the clan" in villages prevented the rise of domestic capitalism, and that power in China was a consequence of control over massive public works systems—in other words, the Asiatic mode of production had existed. The following year, others, including a young Karl A. Wittfogel, expressed essentially similar points of view in print.[7]

Another source often cited by Varga and by other supporters of the Asiatic mode was the work of Russian socialist Georgii Plekhanov (though not a Bolshevik, a mentor of Vladimir Lenin's). Plekhanov, who was also popular among Chinese and Japanese advocates of the Asiatic mode of production, had studied Russian communal property and geography as factors that led Russia along a different historical path from Western Europe. Like Marx, he hoped that peasants acting within the primitive communal form known as the *mir* would propel Russian society past the capitalist stage. His influence among the various advocates of the Asiatic mode of production lay in this emphasis on geographic factors that would lead to distinct Eastern and Western routes of social evolution.[8]

The most prolific Soviet advocate of an Asiatic mode was another Hungarian refugee, best known by his Russified name of Liudvig I. Mad'iar. He served in the Soviet diplomatic corps in China in the mid-1920s and there collected materials for his first major work, *Ekonomika sel'skogo khoziaistva v Kitae* (China's rural economy [1928]), a volume that became one of the central documents in the debate over the next few years. Mad'iar then went to work for the Oriental Secretariat of the Comintern from 1929 to 1934, the year in which he was purged. In 1930, he published a second book supporting his ideas on Asiatic society in China, *Ocherki po ekonomike Kitaia* (Essays on the Chinese economy), and a lengthy introduction to another volume, *"Tszin'-Tian": Agrarnyi stroi drevnogo Kitaia* (*Ching-t'ien*: The Agrarian structure of ancient China) by M. D. Kokin and G. Papaian.[9]

In essence, Mad'iar asserted that the Asiatic mode of production had characterized Chinese society and its economy until the beginning of the twentieth century, when notions of private property came to China from the West. Remnants of the Asiatic mode of production in China were still important in determining the strategy of the Chinese revolution. He and the other "Aziatchiki," as supporters of a distinctive "Asiatic" mode of production were dubbed by their opponents, agreed that Marx had intended to describe not a single historical path for all

[7] V. N. Nikiforov, *Sovetskie istoriki o problemakh Kitaia* (Moscow, 1970), 202–08. See also Sawer, *Marxism and the Question*, 81–82. On Wittfogel's writings and activities in the late 1920s and early 1930s, see G. L. Ulmen, *The Science of Society: Toward an Understanding of the Life and Work of Karl August Wittfogel* (The Hague, 1978), especially 128–41.

[8] Hoston, *Marxism and the Crisis of Development*, 135–38. On Plekhanov's ideas in this connection, see S. H. Baron, "Plekhanov's Russia: The Impact of the West upon an 'Oriental' Society," *Journal of the History of Ideas*, 19 (June 1958): 388–404.

[9] Liudvig Mad'iar, *Ekonomika sel'skogo khoziaistva v Kitae* (Moscow and Leningrad, 1928); *Ocherki po ekonomike Kitaia* (Moscow, 1930); M. D. Kokin and G. Papaian, *"Tszin'-Tian": Agrarnyi stroi drevnogo Kitaia* (Leningrad, 1930), introduction by Mad'iar, 1–75; and "Lajos Magyar (Liudvig Ignat'evich Mad'iar, 1891–1940)," *Great Soviet Encyclopedia* (Moscow, 1974), 15: 321. Liudvig I. Mad'iar's name has been variously mistranscribed by virtually everyone who has mentioned his work: "Ludwig Madyar" by Wittfogel, Dirlik, Ulmen, and others; "Ludwig S. Mad'iar" by Hoston. There is no reason whatsoever to Germanicize his first name.

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societies throughout the world but two. Thus for Mad'iar the Asiatic element in the Asiatic mode of production was not a geographically specific concept but a universal, pre-capitalist stage through which all peoples necessarily would pass.

In their work on the *ching-t'ien* or "well-field" system, Kokin and Papaian actually made use of the standard Chinese sources on ancient society (such as the *Chou-li* or Rites of Chou) in an effort to demonstrate that the Asiatic mode of production had characterized the Chou era (twelfth through third centuries B.C.). The "well-field" system, whose existence has never been proven, was allegedly an ancient institution for dividing land along the lines of the Chinese character for "well," which resembles a tic-tac-toe board. Eight families were said to have worked the eight plots of land surrounding the center for themselves, and the central ninth plot they worked jointly for the state. In their book (translated into Chinese in 1933), Kokin and Papaian portrayed the "well-field" structure as a local commune used by the administrative officers of the state. Because the state owned all the land, rent and tax in this system were united (another important element of the Asiatic mode of production). However, the authors never argued, as their opponents claimed they did, that the Asiatic mode continued into modern times.[10]

The Asiatic mode of production thesis was alternately assaulted and explained away by its opponents, such as Sergei Dubrovskii, who attacked it in 1929 in his work, *K voprosu o sushchnosti "aziatskogo" sposoba proizvodstva, feodalizma, krepostnichestva i torgovogo kapitala* (On the question of the essence of the "Asiatic" mode of production, feudalism, serfdom, and trade capital), soon translated into Chinese and Japanese. He posited ten modes of production through world history, but the Asiatic was not one of them.[11] Many others claimed that the Asiatic mode of production was merely an Asiatic variant of feudalism or slavery but certainly no mode of production unto itself.

The debate reached such intensity that several conferences—in Tbilisi and Baku in 1930 and in Leningrad in 1931—were convened to resolve the issue. The Baku papers were never published, and little has ever been reported on the Tbilisi meeting (a conference volume exists, but it is extremely rare) except that the debate revolved around the views of T. D. Berin in favor of an Asiatic mode. The views against, as represented by Dubrovskii and Mikhail Godes, prevailed.[12] Despite the anti-Aziatchik tenor of these meetings and of the press in 1930, views supportive of an Asiatic mode of production had yet to be silenced.

[10] Kokin and Papaian, *"Tszin'-Tian"*; and *Diskussiia ob aziatskom sposobe proizvodstva* (hereafter, *Diskussiia*) (Moscow and Leningrad, 1931), 57. One anti-Aziatchik attacked the joint work of Kokin and Papaian for "claiming . . . that in the Han era in China private property in land did not exist and was all government land, and that land was neither bought nor sold." Hearing this, Kokin shouted from his seat: "I didn't claim that. Maybe Papaian did." When the laughter subsided, according to the minutes (*Diskussiia*, 57), Papaian said: "I never claimed it."

[11] Sergei M. Dubrovskii, *K voprosu o sushchnosti "aziatskogo" sposoba proizvodstva, feodalizma, krepostnichestva i torgovogo kapitala* (Moscow, 1929), 17–19; and see E. Varga, "Ob aziatskom sposobe proizvodstva," in Varga, *Ocherki po problemam politekonomii kapitalizma* (Moscow, 1965), 366–67.

[12] Nikiforov, *Sovetskie istoriki*, 217; and Ulmen, *Science of Society*, 138. The Tbilisi report was entitled *Ob aziatskom sposobe proizvodstva: Stenograficheskii otchet diskussii po dokladu T. Berina* [On the Asiatic mode of production: Stenographic account of the discussion surrounding the report of T. Berin].

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The Leningrad Conference of February, 1931 was much more famous. Neither Varga nor Mad'iar were invited to represent Aziatchik views, but Kokin and Papaian both attended and were thoroughly lambasted by their opponents. Both sides came heavily armed with quotations from Marx, Engels, and Lenin and brandished them in warlike fashion. The most outspoken critics of the Asiatic mode of production, Evgenii Iolk and Godes in particular, rejected the Aziatchik notion of a bureaucratic ruling elite defined by its function, not its economic class. Politically, they openly claimed, the Asiatic mode of production was dangerous to the Comintern's efforts to spur revolutionary movements among the world's colonial peoples, because a geographically distinct mode of production could arguably render Comintern leadership unnecessary. Godes attacked the Aziatchiki for failing to explain the transition from the Asiatic mode of production to the next stage of history within the categories of class struggle and Oriental society. In short, opponents of the Asiatic mode denounced its supporters for distorting the words of Marx and Engels and exaggerating what was simply an Asian variant of feudalism or slavery.[13]

While the expression "Asiatic mode of production" began to disappear from the Soviet press that year, its previous supporters did not. The Aziatchiki published over the next few years in respectable publications but not directly on the subject of the Asiatic mode of production.[14] A few Aziatchiki even survived Stalin's purges, although most perished. Wittfogel later wrote of the Soviet Aziatchiki that "their heresy was a minor one, and it did not deprive them of their good Communist standing."[15] He also claimed that the Asiatic mode of production was not widely debated in Communist parties outside the Soviet Union. This statement was clearly untrue for both China and Japan.

THE CURRENT NOTION THAT THE ASIATIC MODE OF PRODUCTION attracted little interest in China would be difficult to substantiate.[16] Supposedly, because of its geographical specificity and its inherent quality of stagnation, the Asiatic mode of production had condescending (perhaps even racist) overtones for Chinese radicals. This argument is based more on abstract logic than on sources. One could equally hypothesize an approach, similar to that of the Soviet Aziatchiki, that posited the unique character of the Asiatic mode of production for China and the

[13] *Diskussiia*, 14, 34, 66. See also Ozaki Shōtarō, "Ajia teki seisan yōshiki ronsō," in *Ajia teki seisan yōshiki ron* (Tokyo, 1949), 65–82; Goi Naohiro, *Kindai Nihon to Tōyō shigaku* (Tokyo, 1976), 194–95; Kobayashi Ryōsei, *Ajia teki seisan yōshiki kenkyū* (Tokyo, 1970), 155–62; and Jan Pecírka, "Die Sowjetischen Diskussionen über die Asiatischen Produktionsweise und über die Sklavenhalterformation," *Eirene*, 3 (1964): 147–69.

[14] Kokin, for example, contributed two long essays on recent events in China for volumes edited by Godes: M. D. Kokin, "Revoliutsiia 1911 goda v Kitae," in *Probuzhdeniie Azii: 1905 goda i revoliutsii na vostoke*, M. S. Godes, ed. (Leningrad, 1935), 131–227; and M. Kokin, "Kitai," in *Ocherki po istorii vostoka v epokhe imperializma*, A. Alimov and M. Godes, eds. (Moscow and Leningrad, 1934), 259–363. It was also recently discovered by a Chinese scholar that Kokin was one of the earliest Russian translators of Lu Hsün's "The True Story of Ah Q." He published the translation in 1929 but did so anonymously. See Ko Pao-ch'üan, *"Ah Q cheng chuan" tsai kuo-wai* (Peking, 1981), 52.

[15] Karl August Wittfogel, *Oriental Despotism: A Study in Total Power* (New Haven, Conn., 1957), 404.

[16] Benjamin I. Schwartz, "A Marxist Controversy in China," *Far Eastern Quarterly*, 13 (February 1954): 143–54; and Dirlik agrees with Schwartz on this point, in *Revolution and History*, 191–92, *passim*.

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necessity of a socialist revolution, led by workers and peasants, because China's bourgeoisie was too ineffective to guide a bourgeois-democratic revolution. In fact, the Asiatic mode of production was as widely discussed as any other issue in the debates on the history of society then raging in China, and the debates continued as long in China as anywhere else in the world.

An examination of journals and books of the late 1920s and 1930s reveals that many works of the Aziatchiki and their opponents were translated into Chinese soon after their initial publication, including those of Varga, Wittfogel, Mad'iar, Kokin, and Papaian. Mad'iar's first book was actually translated several times by different publishers. Given the contours of the Soviet debate, the Chinese had access to all of the relevant Soviet materials and much of the earlier works of Marx, Engels, and others.[17]

As was the case in Russia, the Chinese debate on the history of society was closely entwined with revolutionary strategy. Most of the participants in the Chinese discussions were political activists first and historians second, and their historical analyses reflected a primary concern with recounting the development of Chinese society in a way that pointed directly to revolution. Unlike their Soviet counterparts, however, the Chinese debaters soon proposed elaborate schemes and periodizations for Chinese history, occasionally with the use of recent archaeological materials (oracle bones and inscriptions on bronze artifacts).

The highly erratic Kuo Mo-jo, who had translated Marx's *Critique* in 1925, opened the theoretical debate over the Asiatic mode of production in China with his book *Chung-kuo ku-tai she-hui yen-chiu* (A study of ancient Chinese society), first published in 1930 and reissued many times thereafter. In it, he associated the Asiatic mode of production with the primitive-communal social order. Confronted by severe criticism, Kuo changed his views (something for which he became famous), arguing in an essay of 1936 that the Asiatic mode was in fact a distinct class society predating slavery.[18]

Li Chi, a scholar of considerable erudition in ancient history, held views most closely corresponding to Mad'iar's on the nature of the Asiatic mode of production. Li repeatedly stressed the simple fact that Marx had listed the Asiatic mode as one of the normal stages of social development. A Marxist therefore had no choice, in Li's opinion, but to find it in history. Thus, Kuo Mo-jo was wrong in 1930, Li claimed, to identify the Asiatic mode of production with a pre-class society. Li argued that the Asiatic mode best described the late Shang era (through the twelfth

[17] For example, there were the following translations of Mad'iar's first book: *Chung-kuo nung-ts'un ching-chi chih t'e-hsing*, Tsung Hua, trans. (Shanghai, 1930), a partial translation; *Chung-kuo nung-yeh ching-chi*, Li Min-ch'ang, trans., stenographic copy of translation of chapter 12 (Moscow, 1929); and *Chung-kuo nung-ts'un ching-chi yen-chiu*, Ch'en Tai-ch'ing and P'eng Kuei-ch'iu, trans. (1930; rpt. edn., Shanghai, 1934), complete translation with additional material. Also, of Mad'iar's second book of 1930: *Chung-kuo ching-chi ta-kang*, Hsü Kung-ta, trans. (Shanghai, 1933), complete translation.

[18] Kuo Mo-jo, *Chung-kuo ku-tai she-hui yen-chiu* (Shanghai, 1930), 176–77. Kuo's essay in 1936 was "Chung-kuo fa-chan chieh-tuan chih tsai jen-shih: Chu yü lun-chiu so-wei 'Ya-hsi-ya te sheng-ch'an fang-shih,'" in *Mo-jo wen-chi*, 17 vols. (rpt. edn., Peking, 1959), 11: 21–27.

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century B.C.). Relying on a handful of citations from *Capital*,[19] the work of Plekhanov, and especially Lewis Henry Morgan's *Ancient History* (a major source for Marx and Engels), Li also leveled criticism at Mad'iar for implying that the existence of the Asiatic mode of production in China's past meant China had bypassed feudalism.[20]

The historian Hu Ch'iu-yüan argued in several essays published in 1932 that if the Asiatic mode of production was anything, it was despotism, and despotism was grounded in feudalism. He thus claimed that in fact a combination of the village commune and feudal serfdom in China had produced "Asiatic" despotism, and the Aziatchiki (particularly Mad'iar) had failed to see this because they concentrated their gaze on the scholar-official class. Hu was much impressed by the treatment of the issue of the overwhelming authority of the sovereign in East Asia given in 1928 by the Japanese Communist historian, Hattori Shisō.[21]

Two other participants in the debate who arrived at altogether different conclusions about the Asiatic mode of production were Wang I-ch'ang and Liu Hsing-t'ang. Wang criticized Mad'iar for his ignorance of Chinese history, the only possible reason he could imagine for positing a self-sufficient Asiatic mode in China. The Asiatic mode of production itself, he claimed, was not a complete stage in historical development but merely a variant of another (feudalism). Wang's most interesting insight (not an original one, but the first in China) was that India formed the basis of Marx's understanding of Asian social forms, and only occasionally was Marx referring to feudal East Asia.[22]

[19] This was before Kuo Ta-li's complete translation of Marx's text was available. Li often translated directly from German. Others made it clear when referring to *Das Kapital* that they were using the Japanese edition (translated by Takabatake Motoyuki).

[20] Li Chi, "Tui-yü Chung-kuo she-hui shih lun-chan te kung-hsien yü p'i-p'ing," *Tu-shu tsa-chih*, 2 (March 1932): 9–11, 12–13, 28, 47–51, 114; 2 (August 1932): 9, 60; and Li Chi, "Kuan-yü Chung-kuo she-hui shih lun-chan te kung-hsien yü p'i-p'ing," *Tu-shu tsa-chih*, 3 (April 1933): 1–86. As one Japanese critic, Ozaki Shōtarō, noted at the time, Li failed to clarify the links between the Asiatic mode of production and either slavery or feudalism; he just mechanically posited it some time before feudalism and in place of slavery. (See Ozaki, "Ajia teki seisan yōshiki ronsō," 105–06.) In fairness, though, no one before World War II devised a satisfying theoretical framework for the development from pre-class to class society, or from the Asiatic mode of production to the next mode of production, except for the intrusion of Western imperialism into Asia, which was said to have destroyed the self-sufficient economy.

[21] Hu Ch'iu-yüan, "Lüeh-fu Sun Cho-chang chun ping lüeh-lun Chung-kuo she-hui chih hsing-chih," *Tu-shu tsa-chih*, 2 (March 1932): 18–19, 21, 22–23; Hu Ch'iu-yüan, "Ya-hsi-ya sheng-ch'an fang-shih yü chuan-chih-chu-i," *Tu-shu tsa-chih*, 2 (August 1932): 1–4, 6, 8–9, 20–21; Hu Ch'iu-yüan, "Chuan-chih-chu-i lun," *Tu-shu tsa-chih*, 2 (December 1932): 3–4, 22. Hu cited the work of Iolk to substantiate his notion of "Oriental despotism."

[22] Wang I-ch'ang, "Chung-kuo she-hui shih-lun shih," *Tu-shu tsa-chih*, 2 (March 1932): 46, 48–49, 52. Another author, Wang Li-hsi, rejected the idea that China had experienced a qualitatively distinct history requiring a special mode of production to describe. The implication, as Wang read Mad'iar, was that, prior to the imperialist penetration, China was dominated by the Asiatic mode of production; after the intrusion, China's mode of production began to change, but important remnants of the Asiatic mode remained. Ultimately, Wang agreed with Dubrovskii and condemned as anti-Marxist excessive stress on irrigation as a factor in social history. See Wang Li-hsi, "Chung-kuo she-hui hsing-t'ai fa-chan shih chung chih mi te shih-tai," *Tu-shu tsa-chih*, 2 (August 1932): 2–6. Ch'en Pang-kuo, another contributor to the debates, was Li Chi's most relentless, sarcastic, and dogged critic. He repeatedly denounced the notion of the Asiatic mode of production, reserving special venom for Li. See, for example, Ch'en Pang-kuo, "'Kuan-yü she-hui fa-chan fen-ch'i' ping p'ing Li Chi," *Tu-shu tsa-chih*, 2 (August 1932): 4, *passim*.

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Liu Hsing-t'ang made it clear that he was a follower neither of Mad'iar's nor of Li Chi's notion of the Asiatic mode of production, yet he had to agree with both that the lack of private property and the importance of irrigation had greatly influenced East Asian society. Although essentially repeating Dubrovskii's critique of Mad'iar, Liu conceded that Mad'iar had offered much of value to scholarship on Chinese society. In fact, he argued, the village commune (the central element of the Asiatic mode of production) had retarded China's development and prevented a substantial indigenous growth of commerce. In its original and remnant forms, the organization of this village commune (*nung-ts'un kung-she* or *nung-ts'un kung-t'ung-t'i*) was the source of China's social retardation.[23] Although Liu claimed no intellectual filiation with Mad'iar and the Soviet Aziatchiki (perhaps for political reasons), he ultimately did adopt some of their main ideas.

One of the fascinating elements of the debates over the Asiatic mode of production in China was the attribution of importance to certain lines of thought imported from the Soviet Union. For example, we know that the positions of Iolk and Godes at the Leningrad Conference against the Asiatic mode were studied by interested Chinese almost immediately, but it was clear to all concerned that Iolk and Godes knew nothing of Chinese history. But, if loyalty to the movement required Chinese Marxist historians to pay lip service to the victors in Leningrad, an all-powerful party in China could not as yet prevent them from often entertaining (even if they were ultimately unable to accept) the ideas of Mad'iar and other Aziatchiki, whose views on the Asiatic mode of production had simply disappeared from printed Soviet works after 1931. No Soviet writer on this issue was translated as often and as extensively as Mad'iar. A close examination of Chinese writings on the Asiatic mode makes clear that the views of men like Godes and Iolk were dutifully invoked in China but never cited as expert. Their positions simply represented the correct Comintern line.

The two Soviet scholars whose views most influenced subsequent Chinese (and Japanese) discussions of the Asiatic mode of production were V. V. Reikhardt and, particularly, Sergei I. Kovalev. This influence is especially interesting, considering that Reikhardt and Kovalev are scarcely mentioned in the definitive Soviet work on the history of Soviet Sinology. In a volume published in 1934 on pre-capitalist economic formations, Reikhardt discussed at length the Soviet debate of 1929–1931 as the confrontation of views for and against the Asiatic mode. While Reikhardt dutifully claimed to agree with the anti-Aziatchik position that the Asiatic mode of production never existed as a distinct mode of production, he nevertheless devoted considerable space to spelling out what it had meant to Marx and Engels. Having done this, Reikhardt concluded that the Asiatic mode of production could not constitute a variety of feudalism but had to be an Asiatic

[23] Liu Hsing-t'ang, "T'ang-tai chih kao-li-tai shih-yeh," *Shih-huo*, 1 (April 1935): 5, 15; Liu Hsing-t'ang, "Chung-kuo ching-chi fa-chan te pen-chih," *Wen-hua p'i-p'an*, 2 (1935): 206–07; and Liu Hsing-t'ang, "Chung-kuo she-hui fa-chan hsing-shih chih t'an-hsien," *Hsin sheng-ming*, 2 (October 1935): 7, 10, 16, 24, 26–27.

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variant of slavery.[24]

Even more widely read, analyzed, and influential was the work of Kovalev. From his comments at the Leningrad Conference, one can see that Kovalev in 1931 had in fact been a defender of the Asiatic mode. He strictly differentiated it from slavery and feudalism and argued that it was a class society with a ruling elite that exploited agrarian laborers.[25] By 1934, however, Kovalev had changed his views. He now argued that the Asiatic mode of production was not a distinctive stage of social development but an Oriental variety of the slaveholding social order in the West.[26]

A brilliant young historian Lü Chen-yü began to come to terms with the concept of the Asiatic mode of production after reading Kovalev (probably in Japanese translation) in 1934. Although he had earlier been more influenced by the anti-Aziatchik position, Lü changed his views and began to approach Marx's designation of "Asiatic" in the empirical materials available on Shang and Chou slavery. He concluded in the mid-1930s, in harmony with Kovalev's later position, that the Asiatic mode of production was essentially a variant of ancient East Asian slavery.[27]

Lü published two books on ancient China in 1934, both beginning with an essay addressing the issue of periodization and the Asiatic mode. In both, he raised the various views then current on the nature of the Asiatic mode, quoted Marx's preface to the *Critique*, and noted Plekhanov's well-known caveat that Marx probably would have changed his ideas on the Asiatic mode and antiquity had he read Morgan earlier. In one of these books, Lü claimed that Plekhanov was "absolutely correct" and in line with the critical "spirit of Marxism." In the other book, he noted that Plekhanov was "wrong," revisionist, and had influenced Mad'iar, Wittfogel, and others.[28] Both essays proceeded to exactly the same summary of Mad'iar's views on China and the Asiatic mode of production and his exaggerated emphasis on water as an explanatory factor in history.[29]

In 1937, Ho Kan-chih summarized the entire Chinese debate on the history of society to that point. While he criticized "Comrade Mad'iar" and the whole "Asiatic" notion as unsound, he saw fit to do so only after describing Mad'iar's views in great detail. He argued that, although the Asiatic mode of production may have

[24] V. V. Reikhardt, *Ocherki po ekonomike dokapitalisticheskikh formatsii* (Moscow and Leningrad, 1934), 54–58; and Nikiforov, *Sovetskie istoriki*, 250–54. Hou Wai-lu briefly discussed Reikhardt's views in *Chung-kuo ku-tai she-hui shih lun* (Peking, 1955), 14–15.

[25] S. I. Kovalev, in *Diskussiia*, 78–80; and Moritani Katsumi, *Ajia teki seisan yōshiki ron* (Tokyo, 1941), 97–108. Kovalev's influence was entirely theoretical. He was not an East Asian specialist and offered nothing of direct import to the discussions concerning China or Japan. He was, though, well known and influential in the Soviet Union. See Dunn, *Fall and Rise*, 52–53.

[26] This view was apparently incorporated into the first edition of his textbook (1934), but it was lifted in its entirety from the second edition: S. I. Kovalev, *Istoriia drevnogo mira*, 2d edn. (Moscow, 1955). See also Nikiforov, *Sovetskie istoriki*, 250–52.

[27] Ozaki, "Ajia teki seisan yōshiki ronsō," 108–11.

[28] Lü Chen-yü, *Shih ch'ien-ch'i Chung-kuo she-hui yen-chiu* (Peiping, 1934), 12, 14; and Lü Chen-yü, *Chung-kuo yüan-shih she-hui shih* (1934; rev. edn., Shanghai, 1942).

[29] The volume with the positive assessment of Plekhanov has been reprinted several times in China, most recently in 1980: Lü Chen-yü, *Shih ch'ien-ch'i Chung-kuo she-hui yen-chiu* (1961; rpt. edn., Peking, 1980).

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long ceased to exist, its remnants continued to retard the normal development of Chinese society. He noted that the Chinese had been disturbed by Mad'iar's idea that the Asiatic mode continued to influence Chinese social organization until the intrusion of the West in the nineteenth century, but he generously forgave Mad'iar, who had by then "corrected" his views. Ho then introduced the debates among Japanese historians, nearly a decade old at this point, on the Asiatic mode of production.[30] In fact, he devoted considerably more space to developments in Japan than to the Chinese side. Ho's evenhanded and extended treatment of the debate over the Asiatic mode of production is striking, especially considering that he claimed to oppose the idea in principle.

Throughout the 1930s, a large number of Japanese writings on Marxism, history, and Chinese society and history were translated into Chinese. This trend continued even after 1937, when full-fledged war erupted with Japan. One volume that survived the war and has been cited in writings from the People's Republic was a translation of Hayakawa Jirō's *Kodai shakai shi* (A history of ancient society), published in Kweilin in 1942.[31] Hayakawa did not see the Asiatic mode of production as a distinct epoch in social development but rather as a transitional phase between the dissolution of the primitive commune and the emergence of slaveholding in antiquity.

Lü Chen-yü continued to write on the Asiatic mode of production through the early 1940s, when it was no longer widely discussed elsewhere. His extraordinarily broad reading in the relevant Japanese materials and in the Marxist classics made his work of the early 1940s the fullest Chinese statement on the subject to date. He located five perspectives on the Asiatic mode that had by then been advanced in the international debate: that it was a special Oriental path of historical development; that it was not slavery but a mode of production parallel to it, namely, an Asiatic variant of slavery; that it formed a distinctly Oriental feudalism which otherwise followed the universal laws of development; that it was a pre-slaveholding society or a transitional mode of production linking primitive society and slavery; and that the view of Godes and others had "liquidated" Mad'iar's "water theory" and denied the Asiatic mode of production altogether. Lü found fault with all five, for in his opinion only Kovalev's solution opened the way to resolve the issue once and for all.[32]

By the early 1940s, Lü had another difficult hurdle to leap. Stalin had announced that all world history passed through a formulaic sequence of five historical modes of production, and the Asiatic mode of production had not made the list. In a brilliant act of scholarly legerdemain, Lü simply argued that, since Marx and Engels wrote of the Asiatic mode, it could not lie outside Stalin's five (even though it clearly was not there). No contradiction here, for Marxist-Leninist theorists were to blame for the confusion. Marxism-Leninism, he warily noted,

[30] Ho Kan-chih, *Chung-kuo she-hui shih wen-t'i lun-chan* (Shanghai, 1937), 2–3, 10–16, 29–58.
[31] Hayakawa Jirō, *Ku-tai she-hui shih*, Hsieh Ai-ch'un and Yang Mu-p'ing, trans. (Kweilin, 1942).
[32] Lü Chen-yü, "Kuan-yü Chung-kuo she-hui shih te chu-wen-t'i," in Lü Chen-yü, *Chung-kuo she-hui shih chu-wen-t'i*, 4th edn. (Shanghai, 1950; originally published in 1942), 2, 4. This volume of essays has been reprinted (at least) twice: Shanghai, 1954 and 1961.

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offered undoubtedly correct theory, but it could never replace the historical evidence.

Kovalev's later interpretation provided the key to a solution of this conundrum. Kovalev claimed that the Asiatic mode of production existed as a "variant of slavery" in ancient East Asia. Where Stalin had written "slavery," Lü argued that one should read "Asiatic mode of production as a variant of slavery" in the case of China. Lü's own work on the Shang period seemed to demonstrate a slaveholding society with certain "Asiatic" elements, so Kovalev's ideas worked this far. Kovalev went on to suggest, however, that in medieval East Asia certain elements of the Asiatic mode of production remained and forged a distinctive variant of feudalism, and here Lü had to part company with him.[33]

In 1943, the eminent historian Hou Wai-lu included a long discussion of the Asiatic mode of production in a work on ancient Chinese society. Again, he did not openly advocate the Asiatic mode, but, by pointing to the Asiatic qualities of ancient Chinese slavery, he was adopting a position akin to Kovalev's.[34] In a new book in 1948, Hou devoted considerable space to an analysis of the issue in the earlier Soviet, Chinese, and Japanese debates.[35] In 1955, Hou republished this book under a new title and with a new introduction because of renewed debates in China on the nature of ancient Chinese society.[36]

Although little was written in China on the Asiatic mode of production between 1955 and the international revival of the issue in the early 1960s, it should be clear that, contrary to the general impression, it never disappeared, not even during the war years, which was the only hiatus in the Japanese debate. By the same token, no Chinese historian after the mid-1930s supported the concept of a mode of production distinct to Asian or Chinese society. Yet the repeated efforts to come to terms critically with this idea of a mode of production named with geographic specificity in the context of otherwise univerally ascending stages of historical development continued through the 1940s and into the 1950s. It surfaced again in the early 1960s and again in the early 1980s, but, while discussed at length by many and sundry, it has never gained a sustained following.

IN JAPAN, THE ASIATIC MODE OF PRODUCTION gained a considerable following and was debated every year in the leftist scholarly press through the late 1930s. Like their Chinese and Soviet counterparts, the Japanese participants in the debate

[33] Lü Chen-yü, "*Ya-hsi-ya te sheng-ch'an fang-fa* yü so-wei Chung-kuo she-hui te 't'ing-chih-hsing,'" in *Chung-kuo she-hui shih chu-wen-t'i*, 30–44. He also expressed the highest regard for the work of Reikhardt and Hayakawa Jirō, both of whom were heavily influenced by Kovalev. Lü reserved special vituperation for the Japanese scholar Akizawa Shūji. Akizawa published widely in left-wing journals on the Asiatic mode of production and the nature of Oriental society. By the end of the 1930s, his views had clearly evolved into pure and simple window dressing for the Greater East Asia Co-Prosperity Sphere, the Japanese rationale for invasion and occupation of the Asian mainland, by borrowing arguments from the arsenal of the Asiatic mode of production, such as stagnation. Only Japanese imperialism, in his estimation, could remedy it. Lü's labeling Akizawa a mouthpiece for "Japanese fascism" may have been excessive, but his criticism hit the mark. Lü Chen-yü, "Jih-pen fa-hsi-ssu-ti te Chung-kuo li-shih-kuan p'i-p'an," in *Chung-kuo she-hui shih chu-wen-t'i*, 131–49.

[34] Hou Wai-lu, *Chung-kuo ku-tien she-hui shih lun* (Chungking and Chengtu, 1943), 61–104.

[35] Hou Wai-lu, *Chung-kuo ku-tai she-hui shih* (Shanghai, 1948), 13–32.

[36] Hou Wai-lu, *Chung-kuo ku-tai she-hui shih lun* (Peking, 1955), 1.

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were concerned with the fate of the Chinese revolution, as they were with the future of a Japanese revolution. In addition, the Japanese debaters had a more thorough training in Marxism-Leninism than their Chinese counterparts.[37]

The immediate issue of the Asiatic mode of production for Japanese Marxists was tied into the larger debate on the nature of the society that emerged in Japan after the Meiji Restoration of 1868. The Rōnō faction (named for their journal) argued that the restoration was a bourgeois-democratic revolution, while the orthodox Kōza faction (named for a famous collection of essays they published in 1932) saw it as absolutist in character, as neither the bourgeoisie nor the feudal landlord class had led the restoration, allowing an all-powerful state to fill this leadership vacuum. While the Rōnō faction was ready for a proletarian revolution in Japan, the Kōza faction accepted the Comintern determination that Japan needed a two-stage revolution, the first to topple the emperor system and the second the capitalists. As a result, the debate on the Asiatic mode in Japan was almost entirely carried on by Kōza faction Marxists, since it was not of immediate importance to the Rōnō faction.[38]

The central issue for Japanese commentators on the Asiatic mode of production was the enormous role played in Japan's modernization efforts by the state, the imperial institution. The emperor system represented not a progressive historical development but a repressive legacy from the past, something to be overturned. It looked much like an Asiatic remnant in Japan, at a time when such an institution did not exist in China (1920s and 1930s). However, a Japanese critic risked *lèse majesté* if he identified the Asiatic mode of production (as the object for revolution) with the emperor and Japan's top-heavy state system in the post-restoration period, a force of ever more oppression and militarism in the Shōwa era (from 1926). In fact, the Asiatic mode of production became a metaphor for the resistance of Japanese society to development.

The Japanese participants effectively shared among themselves the tasks of translating the pertinent materials from Russian, German, and Chinese, thus eliminating repetition of effort. Their work had a salutary effect on the scholarly level of the controversy in Japan. All the major writings of the Aziatchiki and anti-Aziatchiki appeared through the first half of the 1930s, and a full translation by Hayakawa Jirō of the Leningrad Conference papers appeared in 1933.[39] Because of the political commitment to the Chinese revolution, major writings from the Chinese debates, particularly books by Kuo Mo-jo and T'ao Hsi-sheng, rapidly appeared in Japanese as well.[40]

[37] On the assimilation of Marxism (and Marxism-Leninism) in Japan and all the Japanese factors therein, see Hoston, *Marxism and the Crisis of Development*, 35–54.

[38] Hoston, *Marxism and the Crisis of Development*, 145.

[39] Mad'iar's first book, *Ekonomika sel'skogo khoziaistva v Kitae*, appeared in a Japanese translation in 1931, and his revised 2d Russian edition (minus his introductory chapter on the Asiatic mode of production) in 1934. Mad'iar's second book was translated serially in the early 1930s, finally in full in 1936. Inoue Terumaru, trans., *Chūgoku nōson keizai kenkyū* (1931); 2d Russian edition: *Shina nōgyō keizai ron* (1934); 2d book: Tanaka Tadao and Andō Hideo, trans., *Shina keizai gairon* (1936). Hayakawa Jirō, *Ajia teki seisan yōshiki ni tsuite* (Tokyo, 1933).

[40] Goi, *Kindai Nihon*, 179–83. See, for example, Kuo Mo-jo, *Shina kodai shakai kenkyū*, Fujieda Takeo, trans. (Tokyo, 1938); and T'ao Hsi-sheng, *Shina shakai no shiteki bunseki* (Tokyo, 1929). Their books were

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The Japanese contributors to the debate often combined lives as active Communists with dedicated scholarship, and the abrupt conclusion of the debate over the Asiatic mode in the Soviet Union had no such effect in the Japanese scholarly press. Stalinist efforts to end discussion of the Asiatic mode of production failed in Japan, as they had in China, because Japanese (and Chinese) Marxist scholars felt strongly that they had to fit Marx's generic pattern with native historical development, and both Japanese and Chinese were committed to having a social revolution.[41]

The earliest attempt to explain the meaning of the term "Asiatic mode of production" appeared in a long essay by Fukuda Tokuzō in 1927. Although Fukuda laid out all the available citations from Marx, the best conclusions he could suggest were that "Asiatic mode of production" denoted the primitive village commune, that Marx seemed to equate "Asiatic" with "Indian," and that the topic needed much further study.[42] The first serious effort to place the Asiatic mode of production in Japanese history appeared in Hattori Shisō's *Meiji ishin shi* (A history of the Meiji Restoration, [1928]), the book Hu Ch'iu-yüan later cited with admiration in China. Hattori related Marx's notion of the Asiatic mode of production to the combination of handicraft production and small farmer agriculture in the economy of the Edo period (1601–1868).[43] The first Japanese theoretical treatment of the Asiatic mode of production and its place in Chinese history appeared the following year in an article by Hirata Yoshie, who adamantly claimed that Marx and Engels had not characterized Chinese society by feudalism but by the Asiatic mode *and* that this Asiatic mode was essentially a feudal form of exploitation. In other words, China was Asiatic on the surface but fundamentally feudal at heart. Several months later, the party leader Noro Eitarō similarly explained the Asiatic mode of production as a form of state feudalism.[44]

Through the years 1929–1931, numerous articles touched on the Asiatic mode of production, offering a variety of interpretations. The first article devoted solely to the issue of the Asiatic mode itself appeared in May, 1930.[45] Although the author, Terajima Kazuo, accepted the recent resolution of the Sixth Party Congress of the Chinese Communist Party that China was not characterized by the Asiatic mode of production but was semi-feudal, he maintained that this decision concerned only the realm of practice. The theoretical front left many unanswered

also reviewed in Japanese journals, for example: Suzuki Shun, "Tō Kishō to Chūgoku seiji shisōshi," *Rekishigaku kenkyū*, 1 (December 1933): 120–23; and Shida Fudōmaro, "Saikin no Shina shakai keizai shi kenkyū," *Rekishigaku kenkyū*, 1 (January 1934): 223–27, among many such essays.

[41] In this, I agree with Hoston's recent point (*Marxism and the Crisis of Development*, 153): "Marxist scholars and revolutionary activists [in Japan] firmly believed that the integrity of Marx's social theory required a correct identification of the mode of production that dominated contemporary Japan and China."

[42] Fukuda Tokuzō, "Yuibutsu shikan keizai shi shuttatsuten no saigimmi," *Kaizō* (June 1927): 46, 54; and (September 1927): 62.

[43] Hattori Shisō, *Meiji ishin shi*, in *Hattori Shisō zenshū*, 24 vols. (Tokyo, 1973), 1: 55–56.

[44] Hirata Yoshie, "Shina kakumei to nōgyō mondai," *Shisō*, 86 (July 1929): 72–75, 78; and Noro Eitarō, "Nihon ni okeru tochi shoyū kankei ni tsuite," *Shisō*, 88 (September 1929): 54. Noro was arrested by the police on 28 November 1933 and murdered by them in 1934.

[45] Terajima Kazuo, "Marukusu Engerusu ni okeru 'Ajia teki seisan hōhō' no igi," *Puroretaria kagaku*, 2 (May 1930): 47–59. See also Goi, *Kindai Nihon*, 177.

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and debatable problems. Terajima's view closely resembled Mad'iar's, and the next few issues of the journal in which his essay appeared, *Puroretaria kagaku* (Proletarian science), ran several articles on the Asiatic mode of production and numerous pieces concerning China.

In 1931, Itō Zōhei began his work, *Nihon kokka no seiritsu katei* (The formative process of the Japanese state), by quoting Marx's preface to the *Critique* and asking what this sequence of modes of production meant for the study of Japanese history. What was unclear for the Asiatic (while spelled out for the other modes of production), he argued, were the social and economic systems attached. From this interesting observation, Itō concluded that in Japan the Asiatic mode of production emerged in the primitive communal stage of history and lingered in remnant form through slavery to feudalism.[46] Late that year, Hani Gorō essentially concurred that the Asiatic mode of production leaned either toward the slaveholding or the serf mode.[47] He continued to struggle with this strange concept in a long article through the following year, after gaining access to Mad'iar's first book in translation. His conclusions remained the same: the Asiatic mode of production was basically slavery or serfdom, was linked initially to the decline of primitive society, and represented the earliest stage of class conflict in world-historical development. This last point was new and became more important as the debate proceeded. The Asiatic mode of production in China, Hani argued, was not fundamentally distinct from feudalism.[48]

In 1932, the papers from the Leningrad Conference began to appear in Japanese translation. The political victory of the anti-Aziatchiki in Russia might have aborted Japanese discussion of the Asiatic mode, but the temptation to work through all the intellectual problems posed by this mysterious concept, which seemed to contradict other Marxist notions, proved stronger than the Soviet efforts to strangle debate, and few Japanese scholars were prepared to abandon the Asiatic mode of production altogether.[49]

The debate mushroomed in 1933 and 1934, and three schools of thought emerged. In the first were advocates of the Asiatic mode as synonymous with pre-class society; others saw it as a transitional era from the primitive commune to slavery; and still others regarded it as the first class society. In each of these related points of view, the Asiatic mode of production was not unique to Asian society but part of the universal historical progression. A second school of thought included scholars who continued to maintain that the Asiatic mode was an Asian variant of either slavery or feudalism. In these two positions, both derived from Soviet views, the Asiatic mode of production was geographically specific. Finally, there were scholars who saw the Asiatic mode less as a distinct era bounded by two

[46] Itō Zōhei, *Nihon kokka no seiritsu katei* (Tokyo, 1931), 197, 199, 207, 240.

[47] Hani Gorō, "'Ajia teki seisan yōshiki' no mondai ni yosete," *Teikoku daigaku shimbun* (21 December 1931), as cited in Goi, *Kindai Nihon*, 196.

[48] Hani Gorō, "Tōyō ni okeru shihonshugi no keisei," *Shigaku zasshi*, 43 (February 1932): 1–42; 43 (March 1932): 1–40; 43 (June 1932): 26–58; and 43 (August 1932): 48–58; especially part two subtitled "Ajia teki seisan yōshiki to Shina shakai," 43 (March 1932): 5, 16, 22, 28, *passim*.

[49] Ozaki, "Ajia teki seisan yōshiki ronsō," 88. Akamatsu Keisuke was one of those ready to give up on the Asiatic mode of production altogether.

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others in the historical continuum than as a quality that distinguished the long-term development of pre-capitalist Oriental society from primitive communism through feudalism: namely, stagnation and despotism.

Hayakawa Jirō was an early and consistent contributor to the Japanese debate. In 1933, he was prepared simultaneously to suggest that Japanese society after the Taika Reforms of 645 resembled the Asiatic mode of production, that Mad'iar and Varga had held incorrect positions, and that "of course, the Asiatic mode of production is a kind of feudalism."[50] He came under ferocious criticism that year. Some attacked his idea that placed the Asiatic mode after the Taika Reforms. One author simply asked what had happened to slavery in Japanese history.[51]

In 1934, Hayakawa began to publish work on the Asiatic mode of production in a new vein, now denying that it had anything to do with feudalism; it was this incarnation of his work that was published in Chinese translation. He now regarded the Asiatic mode of production as one social formation that emerged in the process of the dissolution of primitive society. He meant to offer the Asiatic mode of production the dignity of its own place as a bona-fide mode of production, one of Marx's original four, and thus a class society, in fact, the first. He also linked it, citing *Capital*, to a complex system of tribute-bearing vassalage (*kōnōsei*) established when one community conquers another.[52] The reason that Hayakawa's Asiatic mode of production resembled slaveholding is neither coincidental nor because he viewed it as an Asian variant of slaveholding society. Rather, he saw the Asiatic mode as most potentially operative at that stage of social development when one society has conquered a more primitive society and put an unequal system of enforced vassalage into effect. This action would destroy the pristine commune and begin a path toward slavery—and the Asiatic mode fell along this path in Hayakawa's formulation.

In their early contributions, Hattori and Hani agreed with Iolk's position that regarded the Asiatic mode of production as an Asiatic variant either of feudalism or slavery. When he looked back at the debate in the immediate postwar years, though, Hattori chiefly remembered the confusion and plethora of views spawned as texts poured into Japan from Russia and elsewhere. He recalled a comment made in 1935 by one of the lesser-known participants: "It is my feeling that for

[50] Hayakawa Jirō, "Nihon rekishi to 'Ajia teki seisan yōshiki,'" *Rekishi kagaku*, 2 (March 1933): 19–21.

[51] Aikawa Haruki, "Ajia teki seisan yōshiki no Nihon rekishi e no 'tekiyō' ron ni kanren shite," *Rekishi kagaku*, 2 (May 1933): 41, 47; and Aikawa Haruki, "'Ajia teki seisan yōshiki' to Nihon hōkensei ni kansuru ronsō (1)," *Rekishi kagaku*, 2 (Fall 1933): 69.

[52] Hayakawa Jirō, "Tōyō kodai shi ni okeru seisan yōshiki no mondai," *Rekishi kagaku*, 3 (December 1934): 72–74, 79–80; and Shiozawa Kimio, *Ajia teki seisan yōshiki ron* (Tokyo, 1970), 23–27. Hayakawa's early critic, Aikawa Haruki, took a position in 1933 akin to Godes's view that the Asiatic mode of production was an Asian form of feudalism. This was Hayakawa's view in that year, but Aikawa disagreed on important details, such as when it was supposed to have transpired in Japan's past. Late in 1933 and early the following year, his views began to change in a direction curiously similar to Hayakawa's. Now he argued that the Asiatic mode of production constituted history's first class society. By 1935, he had come in contact with Kovalev's later views, which unquestionably influenced him. Aikawa still held to the notion that the Asiatic mode of production was the first class formation, but he added that it was based on a patriarchal slaveholding society. Aikawa Haruki, "*Ajia teki* kannen keitai e no keikō: *Ajia teki seisan yōshiki* to Gōdesu teki kenkai," *Shisō*, 139 (December 1933): 68–69, 77–86; Ozaki, "Ajia teki seisan yōshiki ronsō," 91, 98–99; Shiozawa, *Ajia teki seisan yōshiki ron*, 28–29.

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us to aim toward a proper understanding and explanation [of the Asiatic mode of production], *it is an indispensable precondition that we clearly elucidate the* [appropriate] *documents and methodology of Marxism and in addition that our concrete knowledge of history be filled out considerably.*"[53] In this, Hattori wholeheartedly concurred.

No Japanese scholar involved in this debate made more prolific use of the available doctrinal, as opposed to the empirical, material than did Moritani Katsumi. In 1934, he demonstrated great versatility in handling the writings of Marx and Engels in an essay that placed the Asiatic mode of production before slavery at the final stage of the primitive commune.[54] Over the next few years, he published several more articles on the subject, finally putting them together as a book in 1937. Working with one basic source that he quoted in whole or in part literally dozens of times and usually in boldface—that one sentence from Marx's *Critique*—Moritani claimed that the Asiatic mode of production constituted a full-fledged epoch in the universal societal pathway. If Marx had not meant it as a genuine historical stage, Moritani argued, echoing Li Chi, he would not have stated it so clearly to be one. Moritani identified the Asiatic mode of production with the "agricultural commune" (*nōgyō kyōdōtai*), a technical term indicating a society that emerged from the lower-level, primitive commune. The Asiatic mode of production then constituted a generic social and economic formation in the larger transition from pre-class to class society on the way to slavery and beyond. As such, it could not have been meant to be geographically specific to Asia but rather to be a "category of world history" whose primary qualities had been reached earliest and most clearly in Asia—hence its name.[55]

Ozaki Hotsumi, a famous journalist executed for his role in the Sorge spy ring, closely followed Moritani's explanation of the Asiatic mode of production in a series of lectures given at Tokyo Imperial University in early 1939, published later that year as *Gendai Shina ron* (On contemporary China). For Ozaki, the fact that the "village commune" was responsible for social stagnation was obvious; the really important question was how it had been preserved for so long. His answers were less satisfying: the importance of wet-field agriculture in addition to the great ability of China's social structure (before the intrusion of the capitalist powers) to assimilate conquerors and their lower social and cultural levels.[56]

[53] Hattori Shisō, "Nihon ni okeru Ajia teki seisan yōshiki ronsō no shūketsu" (1948), in *Hattori Shisō zenshū*, 21: 14–17, 19, 21–22, quotation on 22, emphasis in original. The author of the quotation is Tatsumi Tsuneyo.

[54] Moritani Katsumi, "Shina keizai shakai shi no shomondai," *Rekishi kagaku*, 3 (April 1934): 5–6.

[55] Moritani, *Ajia teki seisan yōshiki ron*, 2–3, 52, 63–64, 68, 70, 72–73, 82, 85, 89–90.

[56] Ozaki Hotsumi, *Gendai Shina ron* (1939; rpt. edn., Tokyo, 1964), 31, 33–34. See also Chalmers Johnson, *An Instance of Treason: Ozaki Hotsumi and the Sorge Spy Ring* (Stanford, Calif., 1964), 197. In a letter to his wife from prison, Ozaki returned to the theme of the nature of Chinese society. Dissatisfied with his earlier treatment of the topic, he now felt that China was not thoroughly "feudal," but combined an Oriental despotic state structure and bureaucracy with an emerging feudal society. The designation "semi-feudal" was therefore appropriate. "You're probably not very interested in these things," he admitted in concluding this letter, as he awaited interrogations and eventual execution for high treason, "but I'm making notes to write up on another occasion." His next letter (two days later), returned to the subject of state (Oriental despotism) and society (feudal) in China. "While water control is surely of immense importance, I think some scholars far exaggerate it . . . Trying to explain Egypt, Babylonia, India, etc. as a group and all with 'water' has serious limitations as well as numerous spatial and temporal

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Watanabe Yoshimichi arrived at similar conclusions in his book, *Nihon kodai shakai* (Ancient Japanese society). He argued that Asiatic society preceded antiquity and was based on communal relations. The Asiatic mode of production was also, however, a transitional era, the last stage of primitive communism and the incubus for the formation of a society that recognized private property. In order to posit such a transformation from a pre-class to class society, Watanabe had to generate an explanation for the origin of class differentiation within the undifferentiated commune.[57] If all history was the history of class struggle, the Asiatic mode of production had a singular role to play in societal development, for it marked the true beginning of world history linking pre-class and class society.

Another winding path through a variety of views was traveled by Akizawa Shūji. Initially, he was attracted to Hayakawa's depiction of the Asiatic mode as a tribute-bearing system. From late 1935, he abandoned this approach and rejected the assumption that the Asiatic mode of production was a class society preceding antiquity. Instead, he pointed to slavery as the first class society, labeled Mad'iar's ideas "myth," and now identified the Asiatic mode of production with the "village commune" (*nōson kyōdōtai* or, as he translated it, *Dorfgemeinschaft*), acknowledging that this social formation postdated the primitive commune. Because members of the "village commune" had already begun to engage in individual (as opposed to group) activities, the village appeared to be in transition from common ownership to the sprouts of private ownership. Nonetheless, significant communal aspects remained and continued to influence social structure for some time. In fact, Akizawa asserted, the "village commune" stayed in place for an inordinately long stretch of time, not allowing the process of individuation and atomization, observable elsewhere in world history, to develop unfettered. The primary result was the kind of "Asiatic stagnation" commonly seen in China.[58]

In Akizawa's view, the Asiatic mode of production did not qualify as a distinct stage of history, nor did it necessarily apply only to Oriental society. It did prefigure the first social formation in the historical progression—slavery—and Akizawa was relentless in his criticism of anyone who had placed it elsewhere. He thus vilified all those (Godes, Iolk, and others) who had suggested that the Asiatic mode constituted an Asiatic variant of feudalism, for how could the decline of the primitive commune lead to the formation of feudalism? What had happened to slavery? Ultimately, in a flash of extreme confusion, Akizawa concluded that the Asiatic mode of production was an "Asiatic" slavery, adding, as if to underscore the confusion: "The basic characteristic of 'ancient' Asiatic society is the unique and incomplete development of slavery, constricted by the obstinate persistence of the

problems," an argument foreshadowing criticisms years later of Wittfogel's *Oriental Despotism*. Ozaki Hotsumi, *Aijō wa furu hoshi no gotoku*, in *Ozaki Hotsumi chosakushū*, 5 vols. (Tokyo, 1983), 4: 43–44.

[57] Not unlike explaining the autogenesis of life on earth. Watanabe Yoshimichi, "Nihon kodai shakai no sekai shi teki keiretsu: Ajia teki seisan yōshiki ronsō," in *Nihon genshi kyōsansei shakai to kokka no keisei*, Hara Hidezaburō, ed. (Tokyo, 1973), 19–20; and Shiozawa, *Ajia teki seisan yōshiki ron*, 21–22.

[58] Akizawa Shūji, "Jōdai ni okeru shakai keizai teki kōsei," in *Nihon kodai shi no kiso mondai*, Watanabe Yoshimichi, Itō Kimio, Hayakawa Jirō, and Akizawa Shūji, eds. (Tokyo, 1936), 314–15, 324–25; and Akizawa Shūji, *Shina shakai kōsei* (Tokyo, 1939), vi, 5–8, 18, 89–93.

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village commune."[59] Akizawa was fiercely attacked for his views, especially his notion in the late 1930s that the only resolution to stagnation in China was Japan's invasion. No one reserved more vitriol for him than did Lü Chen-yü, but it is doubtful that Akizawa ever saw Lü's attacks.

WHY WERE SERIOUS HISTORIANS OF ANY NATIONALITY attracted to this strange concept named with apparent geographic specificity, and what explains its continuing attraction? While efforts to eliminate the Asiatic mode of production from Soviet discussions of East Asian history were successful by the early 1930s, the debates in China and Japan were just gaining steam at that point. The issues surrounding the Asiatic mode of production and its apparent importance as a retarding element, either in full-fledged form or as a lingering remnant in subsequent historical stages, were just too important to terminate discussion.

Marxist historiography was only taking root in East Asia when the debates on the Asiatic mode of production began. As soon as native Chinese and Japanese historians confronted Marxism, the enigma of the Asiatic mode of production emerged. And, Marx was no help, for a close look at his writings revealed conflicting signals: the entire Orient was afflicted with the retarding characteristics of the Asiatic mode of production; yet the Taiping Rebellion might spur a social revolution in China and the West (as he wrote for the New York *Tribune*), and Japan had experienced a normal feudal stage of development (as he wrote in *Capital*).

Leszek Kolakowski has pointed out that the Asiatic mode of production seemed to contradict three basic tenets of Marxist theory, which was in part the reason for Stalin's efforts to eliminate it. First, standard Marxism spoke of the primacy of productive forces, whereas the Asiatic mode of production seemed to stress geographic factors; second, Marxism ordinarily emphasized the necessity of progress in the history of society, while the Asiatic mode was tied up with social stagnation; and, third, Marxism underscored the universality of social development, while the Asiatic mode by contrast modified that claim, seeing it solely as a Western phenomenon.[60]

On the surface, the interest of Soviet scholars appears both simpler and more bewildering than that of scholars in China or Japan. The Aziatchiki were the trained Sinologists, who could read and speak the Chinese language, who had lived in China, and who were sympathetic to China. They were concerned with working out the details of the Asiatic mode of production and how it fit East Asian history and society. But because the Soviet debate was so consciously conditioned by the requirements of the Chinese revolution and the Comintern's role therein, it was highly politicized from the start. While Mad'iar and the other Aziatchiki seriously attempted to apply Marx's ideas on "Asiatic society" to China and elsewhere, the response to them at the time was blatantly political. The attacks of Iolk and Godes at the Leningrad Conference clearly never aimed at proving point by point that

[59] Akizawa, *Shina shakai kōsei*, 168–69, 171–76, 183, quotation on 183.
[60] Leszek Kolakowski, *Main Currents of Marxism*, 3 vols. (Oxford, 1978), 1: 350.

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Mad'iar or Varga misunderstood Chinese history or Chinese society. In their estimation, the Aziatchiki misinterpreted Marx and Engels—a far worse error—and thereby endangered the future of the Chinese revolution and presumably the Comintern's role in it.

What remains bewildering is the intensity of the attack on the Aziatchiki in the Soviet Union. The simple answer is that Stalin sought control over the Chinese movement, and so he and his proxies insisted that it was not ready for the socialist phase but had to pursue the more moderate, bourgeois-democratic revolution, and align with the Kuomintang, designated as the party of the bourgeoisie. The Aziatchiki called for a socialist revolution because the influence of the Asiatic mode of production had retarded China's development, China's experience with capitalism was extremely young, and the Chinese "bourgeoisie" was too weak to lead a revolution. The Trotskyists at precisely the same time were arguing that Chinese capitalism was sufficiently developed for its proletariat to proceed with a socialist revolution. Stalin and his supporters did away with both of these bothersome groups, often by labeling the Aziatchiki as Trotskyists.

This explanation goes some way in elucidating what happened to the Soviet debate, but it does not explain the variety of anti-Aziatchik perspectives that appeared in print in the Soviet Union through the early 1930s. Some said the Asiatic mode of production never existed; others said, without documentation, that Marx meant it as an "Asian variant of feudalism"; still others said, also without substantiation, that it replaced slavery in the Asian historical context. Which one was the orthodox Stalinist description? We simply do not know. When Stalin in the late 1930s released his definitive statement of the five stages of universal historical development, the Asiatic mode of production was gone, not to reappear in the Soviet Union until de-Stalinization in the early 1960s. A tentative explanation of the intensity of the sentiment against the Asiatic mode of production mixed with a confusion of refutations would be that those "debaters," most of whom knew little of China, were striving to ally with the side they expected would prevail, to demonstrate concerted and vigorous support for Stalin's position, but that they were unable in the early 1930s to fix on the "correct" anti-Aziatchik position.

The Chinese case is equally baffling, albeit for different reasons. Although the early participants in the debates on the history of Chinese society, from which the debate on the Asiatic mode emerged, were not established historians at the time, they and many others became historians as the years progressed. How is it that Marxism attracted their attention in such a rapid and thoroughgoing manner? It could be argued that the political activism of Marxism-Leninism first caught their eye and that theory followed naturally. In other words, Marxism-Leninism posited a revolution in the immediate future, and history, if properly interpreted, could prove that scientifically. Thus, the scientific pretensions of Marxism in conjunction with an emphasis on revolutionary practice provided the key to its appeal to the Chinese.

This explanation, accurate as far as it goes, only illuminates a part of the larger picture. The work of Marx and Engels on the "Orient" shared all the culture-bound prejudices, misinformation, and general ignorance of late eighteenth and

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nineteenth-century Europe. They often wrote in terms of ethnic stereotypes and used data from travelers to seventeenth-century India to characterize nineteenth-century China. One can easily imagine that the notion of an Asiatic mode of production would nauseate nationalistic Chinese historians in their first encounter with it. Chinese radical thinkers had by then been struggling for several decades to have China accepted as a "normal" country, of course with distinctive qualities but fundamentally participating in the same linear path to a glorious future. Historical materialism offered them this pathway. In fact, if the racist overtones of the Asiatic mode of production could be ignored, it might offer them an express ticket to that destination. Thus Chinese historians wrestled long and hard with the Asiatic mode of production. Some found a place for it in the "normal" historical flow. Others followed the Russian example and described it as an Asian variant of feudalism in China. Few, surprisingly few, denied its validity on the basis of its Eurocentrism.

Arif Dirlik has argued that we have to understand the genuine appeal of historical materialism for Chinese historians in the 1920s and 1930s.[61] The question is why. Did Marxism represent a crystallization of theoretical truth? Did it offer a uniformly optimistic view of the future? Chinese historians of this period were initially captivated by Marxist theory, since historical materialism simultaneously made the parameters of understanding clear, provided a plan of action, and gave order to a chaotic world. Historians were thus intrigued by historical materialism as theory, but they were engrossed by Marxism-Leninism as a political instrument. To paraphrase Mao Tse-tung, politics took command, even then.

Chinese historians did not necessarily, consciously or unconsciously, use historical materialism merely to buttress their political plans. They had no choice but to use the Marxist classics as the occasion required. In part, this is why Reikhardt and Kovalev, of negligible import in the Soviet debates on China (and East Asia, generally), became central in the Chinese and Japanese debates on the Asiatic mode of production. These two Soviet writers enabled the Chinese historians to keep intact all the elements of Marxist historical theory, including the Asiatic mode, while explaining away the not-so-attractive side of the Asiatic mode of production by seeing it as just another name for a "normal" stage (slavery) elsewhere. Thus China retained its integrity as a historical society progressing along regular stages of development, albeit with a certain distinctiveness because of the variant quality of its slaveholding period. Reikhardt and Kovalev offered the Chinese and the Japanese the best of both worlds.

There is something deeper and psychologically more telling at the root of the appeal of Marxist historical theory to the Chinese. Perhaps it is true of everyone, but what was clearly true of Chinese Marxists in the 1920s and 1930s was their need for some absolute ontological security, some approach to the understanding of contemporary society, politics, and history that was beyond question, beyond the mundane realities of the contemporary world. No one doubted the essential validity of Marxist theory, insofar as it was understood. Class struggle, social

[61] Dirlik, *Revolution and History*, 21–36, *passim*.

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evolution through stages of development, and all the basic trappings of historical materialism were never debated; they were simply accepted on faith. By comparison, the issue of a distinct Asiatic mode of production was insignificant in the extreme. In fact, the debate on the history of society, by comparison, was a major triviality, a huge intellectual exercise, as budding Chinese Marxist historians began to expand their capacities as scholars.

The Japanese case presents its own set of enigmas. If the idea of the Asiatic mode of production was offensive to the Chinese, as some have argued, it should then certainly have been to the Japanese as well. Marx had noted that, because of the nature of Japanese feudalism, Japan's social development would probably follow that of the West, and hence Japan was, strictly speaking, not "Asiatic."[62] But no sooner did Japanese Marxist historians tackle the Asiatic mode of production than they launched into an immensely rich debate that produced a wide variety of views. Searches for its existence in Japan's past began at once. Hattori Shisō placed it in the Edo period, while Hayakawa Jirō found it in the mid-seventh century, a thousand years earlier.

The Japanese took to Marxism, much as they had to Buddhism and Neo-Confucianism in earlier times and to industrial capitalism in our own, with extraordinary gusto and intellectual interest. Many thought the revolution was soon to come, but their approach to the study of history does not reveal a dominant political bent. One can see clearly that these young Marxist scholars, primarily of the Kōza faction, were struggling to make sense of change and the lack of it in history, of the meaning of historical stagnation, and the place of the Asiatic mode in history. For the Japanese, the principal issue was the origin of an overwhelmingly powerful Japanese state (the emperor system) since the time of the Meiji Restoration. Here, many Japanese critics found the lingering effects of "Asiatic" society, even if Japan had never experienced a pure Asiatic mode of production or had done so centuries before.

In China, the imperial system had been overthrown in the name of republicanism (or bourgeois democracy in Marxist terminology) in 1911, but a sustained modernization effort had failed. Political devolution, economic deprivation, and social instability were rampant and plain to see. China's apparent inability to forge national unity through political or social integration was consistently blamed on the self-sufficient, undynamic nature of the Chinese village, one of the hallmarks of the Asiatic mode of production, even if few Chinese critics actually claimed that China might ever have been characterized as "Asiatic." Mad'iar's work was popular in China, not so much because he attracted followers, but because he had a deeper understanding of the issues involved in Chinese society and history. He had lived and conducted research there, and even if his ideas found little fertile ground, the focus of his analysis and that of many Chinese scholars were similar.

[62] "Japan, with its purely feudal organisation of landed property and its developed *petite culture*, gives a much truer picture of the European middle ages than all our history books, dictated as these are, for the most part, by bourgeois prejudices. It is very convenient to be 'liberal' at the expense of the middle ages"; Karl Marx, *Capital*, Samuel Moore and Edward Aveling, trans., 3 vols. (New York, 1947), 1: 741n.

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ONE WOULD THINK THAT THE CHINESE AND VIETNAMESE revolutions would retire the concept of the Asiatic mode of production from the Marxist schema for all time, and indeed certain neo-Marxist critics in the West have so argued. Who could still possibly entertain the absurd notion of Asia as stagnant? With all the research of the past two generations, who could still argue, as Wittfogel did in the late 1950s, that water control was the root of a monolithic despotism throughout the Orient? Where was the "rural commune" to be found? Committed Marxist historians in the West, such as Jean Chesneaux, felt a need to bridge this gap between the two Marxes, the universalist and the Orientalist.[63] This goal required ignoring questions that might topple the entire edifice by looking for ways to incorporate the changes of the postwar world into Marx's nineteenth-century schema, much as fundamentalists find sanctions for everything in scripture. It also required considerable sleight of hand, for the overwhelming weight of postwar scholarship flies in the face of most of the basic elements of the Asiatic mode.

Most recently, however, more essential questions are being asked. The Chinese are beginning to examine socialism itself, such as Chou Yang's suggestion that there may in fact be alienation under socialism and Hu Yao-pang's subsequently retracted statement that Marxism may have limited applicability for China. In Japan, there has been considerable discussion of the Asiatic mode of production since the conclusion of World War II, influenced by the development of sociology and the Weberian-Marxist theory of local community (*kyōdōtai*) as well as by postwar European work on the Asiatic mode of production, such as that of Hungarian scholar Ferenc Tökei. Japan has also recently witnessed a concerted scholarly rebuttal of the postwar dominance of Marxism in the Japanese academy, taking different forms in different places.[64] At Kyoto University, where Marxist economics was first taught in Japan, the theory of historical stages has been seriously attacked as of little use for an understanding of China, and many who previously applied developmental schemes to Chinese history are in the process of retracting them.[65] The attackers have left many questions unanswered, many others unasked, and still others poorly addressed, but at least basic issues are now on the table for discussion.

At the same time, the Asiatic mode of production may reappear, as it has over the past few years, for altogether inverted reasons and with curious results.[66] When something as strange as the Asiatic mode of production becomes the object of

[63] Jean Chesneaux, "Le Mode de production asiatique: Quelques perspectives de recherche," *La Pensée*, 114 (January–February 1964): 33–55; "Où en est la discussion sur 'le mode de production asiatique'?" *La Pensée*, 122 (July–August 1965): 40–59; 129 (September–October 1966): 33–46; 138 (March–April 1968): 47–55.

[64] Carol Gluck, "The People in History: Recent Trends in Japanese Historiography," *Journal of Asian Studies*, 38 (November 1978): 25–50; Ferenc Tökei, *Sur le mode de production asiatique* (Budapest, 1966).

[65] See, for example, Tanigawa Michio, *Medieval Chinese Society and the Local "Community,"* Joshua A. Fogel, trans. (Berkeley, Calif., 1985), especially 50–59.

[66] See T'ien Jen-lung, "Chien-kuo i-lai Ya-hsi-ya sheng-ch'an fang-shih wen-t'i t'ao-lun tsung-shu," *Chung-kuo shih yen-chiu*, 3 (1981): 147–59, especially 158–59. Recently, an extremely sophisticated treatment of Marx's ideas on the development of non-Western societies was published in China: Lin Chun, "Marx's Conception of the Non-Capitalist Route (1870s–1882)," *Selected Writings on Studies of Marxism*, 5 (1985): 1–23.

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debate in modern China, a society in which the press is so closely controlled, we are well advised to look for another message. The Asiatic mode can be an important vehicle for Aesopian criticism. Through a discussion of the Asiatic mode of production, for example, one can advance a thinly veiled criticism of the tremendous despotic power of the state or its ruler (for example, Mao Tse-tung). Or, it might be used implicitly to buttress the notion of China having a distinctive path to socialism. Or, a Chinese historian may be testing how far he or she can stretch the boundaries of accepted Marxist theory. These purposes appear to have been behind the debate in 1980–1981 on the Asiatic mode of production in China. It is precisely because of the Asiatic mode's unsolved nature within historical materialism that it can be raised and lowered for debate, used as a metaphor for something more important and beyond the ken of direct, public discussion (as it was for the emperor system in pre-war Japan). It seems to appear on the Marxist scholarly agenda during periods when a Marxist orthodoxy is just taking form, is breaking down, or does not exist at all.

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The Crusade against Al-Azraq: A Thirteenth-Century Mudejar Revolt in International Perspective

ROBERT I. BURNS, S.J.

THE THEME SET BY THE MEDIEVAL ACADEMY OF AMERICA for its sessions in April of 1986 was the art of interpretation, specifically, the device of interpretive models in their sequential phases of discovery, proposal, criticism, and modification. More simply, contributions were to deal with "some aspect of interpretation."[1] An episode ripe for fresh consideration is the revolt of al-Azraq at mid-thirteenth century, in the Kingdom of Valencia, then recently wrested from the Spanish Muslims by James the Conqueror. The revolt presents a tangle of problems and is avoided by historians because its apparently local and anecdotal nature hardly seems to justify the labor of explicating and resolving the details. But, if al-Azraq mounted a relatively negligible and local military episode, doomed from the start and soon contained, how is it that the revolt raised serious concern at Rome and brought military support from the pope in explicitly crusade terms?[2] This article is not concerned with the wider semantic or philosophical problem as to whether the Iberian wars of the Reconquista are, strictly speaking, "crusades," although the thirteenth-century popes certainly saw them as crusades equivalent to those in the

[1] Address delivered at the sixty-first annual meeting of the Medieval Academy of America, at the University of New Mexico, 17–19 April 1986.

[2] Míkel de Epalza has gathered virtually all the bibliography on al-Azraq in the course of his *Moros y Moriscos en el Levante peninsular (Sharq al-Andalus): Introducción bibliográfica* (Alicante, 1983), with its continuation in each issue of *Sharq al-Andalus: Estudios árabes*, 1 (1984) and following. These include pioneers such as Roque Chabás and Julián Ribera y Tarragó as well as current contributors such as F. de P. Momblanch y Gonzálbez and M. D. Barceló Torres cited below, and the recent work of Ricard Banyo i Arminyana, Ramón Ferrer Navarro, Enric Botella i Oltra, and Pierre Guichard. See items 91, 105, 107, 167, 202, 203, 229, 378, 379, 412–13, 538, 559, 560, 640–42, 648, 655, 1043, 1045, 1048, 1049, 1053, 1064, 1069, 1108, 1161, 1183, 1217, 1224, 1336, 1410, 1483, 1551, 1552, 1554, 1589, 2182, 2196, 2214, 2229, 2308, 2312, 2314, 2317, 2348. See also Guichard's chapter in the general history of Valencia, *Nuestra historia*, 7 vols. (Valencia, 1980), 3: 27–33; and M. D. Barceló Torres, *Minorías islámicas en el país valenciano: Historia y dialecto* (Valencia, 1984), 64. I dealt at length with al-Azraq in my *Islam under the Crusaders: Colonial Survival in the Thirteenth-Century Kingdom of Valencia* (Princeton, N.J., 1973), 37 and following, 323–32, and index under "Azraq." On the Valencian crusade itself, see my *The Crusader Kingdom of Valencia*, 2 vols. (Cambridge, Mass., 1967), revised as *El Reino de Valencia en el siglo XIII (iglesia y sociedad)* (Valencia, 1982); *Medieval Colonialism* (Princeton, N.J., 1975), revised as *Colonialisme medieval* (Valencia, 1987); *Jaume I i els valencians del segle XIII* (Valencia, 1981); *Moors and Crusaders in Mediterranean Spain: Collected Studies* (London, 1978); *Muslims, Christians, and Jews in the Crusader Kingdom of Valencia* (Cambridge, 1984 and 1986); *Society and Documentation in Crusader Valencia* (Princeton, N.J., 1985); and "The Missionary Syndrome: Crusader and Northwest Pacific Religious Expansionism," *Comparative Studies in Society and History* (forthcoming); with the detailed bibliographies in each.

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Holy Land, and as the western front of a broadly perceived struggle against Islam. As I shall demonstrate, the "revolt" was a major war, an Islamic countercrusade that drew the attention of Christendom and became on the Christian side a formal crusade.

The one sustained narrative of the revolt, sixteen short chapters in the autobiography of King James I, ruler of the Realms of Aragon, provides the scholar with several problems of chronology. "Jaime" to his subjects of upland Aragon, and "Jacme" or "Jaume" to his coastal-urban Catalan subjects, he will be referred to as Jaume here while involved in his more Catalanized kingdom of Valencia. The al-Azraq tale in his memoirs seems artless and straightforward. After his crusade of over a dozen years (1232–1245) had finally won this extensive Islamic "kingdom," Jaume had traveled up to highland Aragon, the older kingdom that formed with Catalonia the core of his realms. News soon arrived in Aragon that "al-Azraq has taken some castles from you in Valencian lands."[3] Angry at this betrayal but pleased by the option it gave to void the surrender constitutions and expel Muslims en masse, the king returned to Valencia, scolding there a conciliatory embassy from the principal Mudejar town, Játiva. Undeterred by the king's presence, the rebel al-Azraq seized Penáguila castle, a place of "importance and honor."[4]

King Jaume summoned a council at Valencia city, proposing to the barons, citizens, and clergy his plan to reinforce the region's castles with 400 knights. Treating the kingdom's Muslim population as a unit, he intended to expel the non-rebels and reduce the rebels by war. The clergy and citizens offered such strong support for their king's project that he was able to override the resistance of his barons, especially the many who had Muslim tenants. Jaume established his garrisons, issued an ultimatum in Arabic, waved aside an offer by the major Islamic *aljamas* to pay a great ransom, and watched as more Muslims revolted "wherever they could" in various "sections of the Kingdom of Valencia." As a dozen castles fell to the rebels, a stream of less bellicose Muslims fled to the Islamic stronghold of Montesa in southern Valencia. "And there was a great war." The peaceful Muslims, a hundred thousand strong according to the king's narrative, were escorted in a line five leagues long to go into exile toward Castile and Granada.[5]

In the second phase of the revolt, the remaining Muslims "made al-Azraq their leader." In northern Valencia, these forces defeated an invading Christian host of 3,000, killing 400 or 500. In the south, they besieged Peñacadel or Benicadell castle

[3] Jaume I, *Llibre dels feyts*, ed. Ferran Soldevila in *Les Quatre grans cròniques* (Barcelona, 1971), chaps. 361–76; 361: "Aladrach vos ha preses alguns castells en terres de València." See also the facsimile edition of the best available manuscript, *Libre dels feyts*, ed. Martín de Riquer (Barcelona, 1972). The Latin version of 1313, *La Crónica latina de Jaime I*, ed. M. D. Martínez San Pedro (Almería, 1984), as posterior to the Catalan is useful here only to confirm or clarify an interpretation. The English translation by John Forster, *The Chronicle of James I*, 2 vols. (London, 1883) is misleading; I am preparing my own translation. On authorship, structure, stages of composition, and especially Islamic influence, see my *Muslims, Christians, and Jews*, appendix 1.

[4] Jaume I, *Llibre dels feyts*, chap. 363: "de tan bon lloch e de tan honrat."

[5] Jaume I, *Llibre dels feyts*, chap. 368: "allí on ells podien"; "per partides del regne de València"; "e hac-hi gran guerra."

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(later the Carbonera region), threatening essential Christian communications. The king's counselors restrained Jaume from personally leading the southern column, citing the many Muslim troops, their high morale, and a mountainous country unsuitable to cavalry operations. The royal army did eventually succeed in relieving the siege, killing a major rebel leader, Ibn Baṣṣāl (Abenbaçol), and driving the enemy further into the mountains around al-Azraq's home base at Alcalá. The king's account at this point grows hurried and vague.[6]

In the third phase, the war lasted "three or four years more," until al-Azraq's overtures to Alfonso the Learned of Castile brought a measure of protection and a year's truce. Jaume used the truce to undermine al-Azraq by suborning the rebel's close counselor, whose advice then left al-Azraq without grain to feed his troops. Jaume rendezvoused with his host at Easter and mounted his final campaign, taking eighteen strongholds in one week and performing prodigies that could, he writes, fill a book. The royal memoirist says little about his reverses, except for one frightening episode when al-Azraq ambushed the king and nearly killed him. In the treaty that ended the war, al-Azraq agreed to leave the country forever, turning over his remaining major castle to his family.[7]

There are many things wrong with this spirited account of Jaume's. The events as briefly covered actually extended over a period of a dozen years or more, a period almost as long as all the campaigns of the original crusade. Al-Azraq's war was not an incidental coda to that crusade but a kind of conquest in counterpoint. Why then did the king airily mask from his readers its true nature? Second, the king's account is oddly achronic or timeless. It is difficult to tell from it the year for any given episode or even the central years in which the revolt occurred. The content is also strange: it is highly improbable that the mass expulsion took place, and the military events are too few and jejune to fill the passage of years or even to explain the negotiated conclusion to the war. Finally, the entire structure of the king's autobiography breaks down: a lacuna of nineteen years (from 1245 to 1264) displays only these few episodes, until the newly confident king resumed his epic story. In these episodes, too, the tone grows bitter toward the Muslims, vindictive in a way foreign to his previous account.

An examination of contemporary or near-contemporary chronicles yields little more about the rebel al-Azraq. Historian-memoirist Ramon Muntaner (1265–1336), when a child, knew King Jaume as a visitor to his father's house. Muntaner's long and detailed chronicle omitted al-Azraq altogether. After a chapter on the conquest of Valencia, he described the far more serious revolt of 1275, in which his hero, Pere, played the major role, first as prince and then as king. Muntaner described that revolt out of sequence, placing it before the 1266 crusade against Murcia; later, he described only a great battle against the Valencian Mudejars just before King Jaume's death. The placement of two sets of troubles owed something to the sequence of topics Muntaner was constructing, so that he

[6] Jaume I, *Llibre dels feyts*, chap. 370: "faeren cap d'Alaçrac."
[7] Jaume I, *Llibre dels feyts*, chap. 371: "durà la guerra bé per tres anys o per quatre."

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had to double back in the telling. It also probably reflects his awareness that two sets of troubles occurred during that thirty-year period.[8]

The historian and memoirist Bernat Desclot was a junior contemporary of King Jaume; he is usually identified with Bernat Escrivà, active in Prince Pere's household from 1268 and later Pere's treasurer. Desclot gave only a paragraph to al-Azraq but placed him in the proper time and sequence. Unlike King Jaume, Desclot devoted more than three times the space given for al-Azraq to a Muslim rebel of the same troubles, "King" Albacor or Albocar, whom he considered a greater man. "Black" and a "shepherd," his name may be al-Baqqār (implying "cattleman" or "herder"). Albacor "did great evil and great damage" in his attacks on the Valencian Christians "and especially to the town of Alcira." He had "strong castles" in the Alcoy-Albaida regions and "many horsemen and infantry." Only when Albacor's campaigns ended with his death did al-Azraq succeed to his mantle.[9]

Albacor would seem to have been one of those "many Saracen barons," presumably local military leaders thrown up by the disintegrating Almohad society, described by Muntaner in his account of the original conquest of Valencia, with whom King Jaume made "truces" or, rather, treaty-agreements so that he could settle the places he held. "And all those with whom he made the said agreements" answered to the king for the annual revenue at those places.[10] The mid-fourteenth-century chronicle of Pere III, called the chronicle of San Juan de la Peña, describes how these "many Saracens holding castles in that [kingdom] remained" after the conquest, later rebelling against Jaume. Some of these leaders may well have been broken during the al-Azraq decade, but many remained to lead the more extensive countercrusade in 1275. The chronicle, indeed, concentrates on their role in that later revolt. King Jaume was obsessed by al-Azraq, the most stubborn and successful of the rebels in the early uprising, and does not even mention "King" Albacor. Contemporary chronicles, then, only deepen the mystery.[11]

If one looks to the enemy side for context and clues to the untold story, it appears that Arabic historians knew nothing and have left nothing about al-Azraq. Arabic charters are also lacking, except for al-Azraq's original surrender or truce in 1245

[8] Ramon Muntaner, *Crònica*, ed. Soldevila, *Cròniques*, chaps. 10, 26–27.

[9] Bernat Desclot, *Llibre del rei En Pere*, ed. Soldevila, *Cròniques*, chap. 49: "era pastor e era negre, molt valent hom"; "feïa gran mal e gran dan a crestians e màgerment a la vila d'Algezira"; "tenia de forts castells . . . e havia molts hòmens a cavall e a peu."

[10] Muntaner, *Crònica*, chap. 9: "ab molts barons sarraïns que havia en lo dit regne, ell féu treves . . . e tots aquells emperò ab que ell féu les dites treves li responien de cosa sabuda l'any."

[11] *Crònica general de Pere III el Cerimoniós*, ed. A. J. Soberanas Lleó (Barcelona, 1961), chap. 36: "en lo dit regne de Valencia romanguessen molts sarraïns tinent castells en aquell, ells emperò aprés alcuns temps se llevaren e s'alçaren contra lo dit sant rei." Compare the Aragonese version, *Crònica*, ed. Carmen Orcastegui Gros (Zaragoza, 1986), chap. 36. Soldevila noted that Jaume's *Llibre* confusingly includes in the al-Azraq troubles the later Castilian war against its Mudejars and Granada in the early 1260s to retain "tota l'Andalusia"; *Pere el Gran*, 2 parts in 4 vols. (Barcelona, 1950–62), 1: 118–19. On the relation between the literary structure of Jaume's autobiography and historical reality here, see my "Al-Azraq Trapped: Jaume's Memoirs Break Off," subchapter in my *Muslims, Christians, and Jews*, 278–81, as well as "The King's Autobiography: The Islamic Connection," 285–88; on the Mudejar "lords" coopted as though part of the feudal system, see 27–32.

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and for his letter of 1250 to Jaume's queen, introducing an embassy.[12] Jerónimo Zurita, the archival historian of the sixteenth-century realms of Aragon, failed badly here, drawing almost exclusively on the king's own account and misdating its events as 1254 through 1256. Even his less reliable colleagues, Pere Anton Beuter and Francesc Diago, recognized, from the term of office of Bishop Arnaldo or Arnau of Valencia at the king's parliament of expulsion, that the revolt had to have begun by late 1247 or early 1248. A modern historian as careful as Charles de Tourtoulon, in his classic biography of King Jaume, did follow the Beuter-Diago argument for 1247/1248 (an argument Francisco Momblanch recreated again very recently) but then ended the revolt abruptly in early 1253.[13]

Historians have had one other major source from which to seek context: a codex of *Repartiment* or land distribution notes, which shows a confused jumble of grants for 1247 through 1249. These grants obviously correlate with the revolt, although it is not clear precisely how. The *Repartiment* includes datelines clarifying the Christian siege of Luchente, incidental information suggesting bodies of troops and displaced Muslim leaders, and a geographical distribution suggesting regions (especially in southern Valencia) presumably much affected by the war and expulsion. Local historians have exploited this material, if clumsily, and to date no one has systematically collated it to illuminate the war. Ramón Ferrer Navarro has recently explored its data for those Alicantine regions around al-Azraq's center of strength.[14]

A major breakthrough came with Joaquim Miret i Sans's monumental *Itinerari de Jaume I*, a collection of charter datelines with basic documentary information, showing the king's whereabouts on any given day; it includes most of the surviving references to al-Azraq.[15] A reconstruction of basic events appeared in Soldevila's edition of Jaume's memoirs.[16] Using Beuter, Zurita, the king's memoirs, and Miret, and drawing from Ambrosio Huici's collection of all the published documents of Jaume (as well as a few new documents), Momblanch expanded the traditional

[12] M. D. Barceló Torres, "Documentos árabes de al-Azraq (1245–1250)," *Saitabí*, 32 (1982): 27–41. R. I. Burns and Paul Chevedden, "El Tractat de rendició d'Al-Azraq amb Jaume I i l'infant Alfons en 1245: El Text àrab i el context valencià," *Homenatge al Professor Manuel Sanchis Guarner* (Valencia, 1983) [= *L'Espill*, 17–18 (1983): 231–57]. A corrected and much more extensive version of our study, in effect, a new article, will soon appear in *Der Islam* in English. The 1250 document is in Maximiliano Alarcón y Santón and Ramón García de Linares, eds., *Los Documentos árabes diplomáticos del Archivo de la corona de Aragón* (Madrid, 1940), doc. 154 (10 March 1250). On its context, see below, n. 47 and text.

[13] Jerónimo Zurita, *Anales de la corona de Aragón*, Angel Canellas López, ed., 8 vols. (Zaragoza, 1967–), and Antonio Ubieto, *et al.*, eds., 3 vols. (Valencia, 1967–), chaps. 50–53. Charles de Tourtoulon, *Etudes sur la maison de Barcelone: Jacme Ier le Conquérant*, 2 vols. (Montpellier, 1863–67), revised in translation by Teodoro Llorente, *Don Jaime I el Conquistador*, 2 vols. (Valencia, 1874), 2 (lib. III), c. IX, esp. n. 1 with citation to Diago (d. 1615), *Anales del reino de Valencia*, c. 237; and see Soldevila's similar argument, *Llibre dels feyts*, 337 n. 2 for chap. 364. Soldevila holds 1248 as the date for Jaume's council at Valencia city (*ibid.*, 337 n. 4 for chap. 365). For Momblanch, see below, n. 17.

[14] *Llibre del repartiment de València*, Antoni Ferrando i Francés, *et al.*, eds., 1 vol. plus 3 in facsimile (Valencia, 1978); and *Libre del repartiment del regne de València*, M. D. Cabanes Pecourt and Ramón Ferrer Navarro, eds., 3 vols. (Zaragoza, 1979–). Ferrer Navarro, "La Repoblación de una comarca alicantina: La Serranía de Alcoy," *Anales de la Universidad de Alicante: Historia medieval*, 1 (1982): 9–27; and "Repoblación de tierras alicantinas por Jaime I," *ibid.*, 2 (1983): 33–48.

[15] Joaquim Miret i Sans, *Itinerari de Jaume I "El Conqueridor"* (Barcelona, 1918), 201, 244, 273–75, 278, 279, 311, and background *passim* from 186 to 296.

[16] *Llibre dels feyts*, notes, 336–41.

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reconstruction of the revolt's chronology.[17] But the several calendars of Jaume's realms, especially the Florentine incarnational and the Spanish era, demand interpretive choices and thus obscure the chronological outline.[18] And the documentary references tend to cluster around the years 1248 and 1258, echoing an earlier tendency to see two distinct and somewhat unrelated revolts.

WHAT METHODOLOGIES MIGHT NOW INCREASE KNOWLEDGE of the episode? Archaeology of Valencian castles is in its infancy, although the work of Karl Butzer and his team on the Eslida site may have some relevance.[19] Literary criticism can reveal something. The king's memoirs, reflecting Islamic models, are a program of military "deeds" or victories. They fall into two natural divisions, on either side of the al-Azraq era, with the missing decades of that era betokening the king's defeats, incompetence to resolve the basic stalemate, and final convulsive victory. The great crusader of Christendom, invited to instruct the prelates and worldly powers at the ecumenical council of Lyons in 1245, had returned to an embarrassing and interminable replay of his original wars.

Close analysis of Jaume's text is rewarding. The revolt was obviously an affair continuing unbroken throughout the entire time of this narrative section, whatever the pattern of local military action. Al-Azraq had precipitated the war, the king widened it by his threats of expulsion, and the Muslims from various regions of Valencia rallied on al-Azraq. The revolt began without North African help, Jaume records, but both sides were aware that this alliance might one day come. Jaume also anticipated a flood of Christian volunteers "from our realm[s] and from other lands." The mountainous southern regions of Valencia were the main focus of the rebellion, the rebels "many," and the war "great."[20] In all, this was hardly the minor or local episode historians have reported. Indeed, if the basic structure of the king's memoirs is an epic story of the Balearics crusade, followed by the epic of the Valencian crusade, this section on the revolt is a parallel if pathetic epic in miniature of the king's victory over al-Azraq.[21]

Widening circles of context can also illuminate the data. In a separate study, I have argued that the Valencian crusade never ended. Instead, al-Azraq and his mountain Muslims in 1245 had momentarily exhausted their diplomatic possibilities, as the civil war threatening Jaume was for the moment healed and as the

[17] Francisco de P. Momblanch y Gonzálbez, "El Rey D. Jaime y las guerras de Alazrach," *Asamblea de cronistas oficiales del reino de Valencia*, 7 (1970): 13; and the revised version as a booklet, *Al-Azraq, capitán de Moros: Aportación a la historia del reino de Valencia en el siglo XIII* (Valencia, 1977), 67–68; Ambrosio Huici, ed., *Colección diplomática de Jaime I, el Conquistador*, 3 vols. (Valencia, 1916–22); expanded rpt., M. D. Cabanes Pecourt, ed., *Documentos de Jaime I de Aragón*, 4 vols. (Valencia, 1976–).

[18] See my *Society and Documentation*, chap. 14, chronology.

[19] See the articles of André Bazzana and Pierre Guichard in Epalza (above, n. 2), and the long archaeological monograph by K. W. Butzer, *et al.* on the Eslida region, *Viator*, 17 (1986): 339–420.

[20] Jaume I, *Llibre dels feyts*, chaps. 368 ("gran"), 370 ("los moros són molts"). On the volunteers, see below, n. 35 and text.

[21] It is noteworthy that the kind of epic verse, encountered previously as prosified in Jaume's *Llibre* but missing for a long stretch previous to the al-Azraq account, can be discerned again in chap. 370, as the Peñacadel and other actions take up again the heroic motif.

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ILLUSTRATION: Muslim army besieging Christian town. A gravity-driven artillery piece (trebuchet) is prominent in the foreground, with massed crossbowmen firing. Under a protective mantle, sappers are eroding the wall. The tent encampment (higher, as a form of perspective) has "behind" it pot-helmeted cavalry in European-style equipment, led by two religious figures in turbans. The Virgin, saints, and angels look down from heaven, prepared to intervene. From the thirteenth-century *Cantigas de Santa Maria* of King Alfonso X the Learned, of Castile, *circa* 1250, El Escorial Library (Patrimonio Nacional).

wider Islamic world in Granada and North Africa fell into deeper confusion. Simultaneously in that year, King Jaume was unexpectedly offered his last chance to recover Provence for his dynasty and so had to cut short his slogging progress in the Valencian mountains.[22] Thus al-Azraq was allowed a three-year truce, while Jaume blithely announced to Christendom the successful close of his crusade, receiving accolades from abroad. My chronological reconstruction argued for 1245 as the date of the king's treaty with the Muslim rebel, not 1244, so that

[22] See my *Muslims, Christians, and Jews*, chap. 10, "Voices of Silence: Al-Azraq and the French Connection: Why the Valencian Crusade Never Ended"; and the details for the central episode in my "The Loss of Provence, King Jaume's Raid to Kidnap Its Heiress (1245): Documenting a 'Legend,'" *XII Congrès d'histoire de la couronne d'Aragon* (Montpellier, 1986), forthcoming; the crusade project to Romania (Byzantium) is also documented there.

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al-Azraq's revolt after only two years seemed to the king the "treason" he repeatedly claimed it to be. The humiliation in Provence followed by the unexpected revolt, followed by an unending stalemate in the south plus other domestic and foreign distractions aplenty, disrupted Jaume's life as well as memoirs. These years were the dark night of his soul.

The *Repartiment* data holds some promise, if properly exploited. Unfortunately, it is confused in its chronology; items marked under "1248," for example, range from 1243 to 1245, and the heading "1249" starts with a document of 1247. Since individual items can lack a date, these chronological groupings are often suspect. Similarly, the geographical groupings do not always correspond to headings and need editorial clarification. Any number of items are repeats, so that simple counting does not avail. Correspondence between charter and actual settlement is unknown. Certain items may represent either pre-grant promises or post-battle confiscation. Above all, there is no guarantee of completeness, since the *Repartiment* may record items of particular relevance to the crown, such as favorites or trouble-spots. There is only this one relevant codex, the second of three constituting our surviving *Repartiment* lists.

The *Repartiment* items seem to show extraordinary activity for 1247–1249, although comparative information is lacking for succeeding years. Still, a review of the evidence reveals strong if relatively minor activity in land grants for 1247 in these districts: at Cárcer (in April–May–June but more in August); Onteniente (March–June–August but more in July); a bit at Pego (April to June, with a large settlement in November); Olocaiba, three items (April–May); Segarria, eight items (April–May); Perpunchent, a single settlement of nineteen people (May); Laguart, two groups of ten in August; Bocairente, some ten settlements (May), in groups of thirty, thirty, twenty, twenty, and several of ten. In the north, Murviedro had a good deal of activity from May to December; and the Alfandech Valley had at least two large settlements (April–July) of ninety-nine and forty crossbowmen. All these activities may be routine, despite the obviously military nature of some of the groups, with no direct relevance to the revolt except as part of the entire post-crusade provocation that Christian immigration meant to the native Muslims.

The pace picks up for 1248, again centering on the spring months and on the southern regions, with thirty instead of ten places affected. Játiva has the lion's share of settlement grants, mostly from March through June. This flurry of grants, plus a tendency by Muslim historians to set 1248 as the date for Játiva's original fall to the Christians (demonstrably not true) suggests that some sort of displacement or "expulsion" took place here, following the scheduled transfer of Játiva's last defenses in 1246 and followed both by a significant royal constitution establishing Christian Játiva in mid-August of 1250 and by its Mudejar constitution in January of 1251 (or 1252).

Even though Játiva does not seem to have involved itself directly in this war, some disturbance must have centered on it as the major nucleus of Valencian Mudejarism, since Jaume speaks of its Muslims in 1247 as "those of Játiva who had defaulted" in their feudal obligations. The king's Catalan term *forfeit* echoes the feudal *forisfacere* in his realms, a detestable crime leading to confiscation. There

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may even have been some demonstration in force by the king, with a collapse of local autonomy ensuing. Considering Játiva's continuing Mudejar strength and the late date of its constitutions, however, this early settlement may partly have been takeovers by landlords (reducing Muslims to direct tenantry) and partly a liberal lacing of immigrants and immigrant groups (groups of thirty, sixty, and thirteen households) as the Muslim affluent and artisans left the country while the bellicose went off to join al-Azraq. If so, this influx was the beginning of the official takeover by the Christian settlers in 1250. The month of March, 1248, saw a sudden flood of over forty grants, with continuing high numbers from April through July. Castellón de Játiva had a large influx in April–May.

Guadasequies saw most of its settlements come in March, 1248. Albaida Valley had its increment especially in May–June, including a group of nine knights with ninety-three infantry, another of thirty, and others of twenty-five or ten. Luchente, subject of a major siege of the Muslims by the king, received surprisingly few settlements at the end of the siege. Some places on the list had only nominal or very modest settlements: Gandía (mostly in June), Jalón, Penáguila (three in August), Navarrés, Palma, Borró, Altea (a group of forty in May), Callosa, Jeresa, Onteniente, Alcoy (two in August), Enova (thirty in one group, ten in another), Cárcer, Negret, and so on. Alcira, long consolidated and Christian, had some twenty grants in April. These figures may easily correspond not to some large-scale expulsion but to substitution of Christian authority for Muslim, with an understandable drift out of the country in these unsettled times, combined with a royal policy of strategic immigration.

Repartiment records for 1249 are closer in volume to those of 1247 than of 1248, especially since many of that year's localities were at the north or center, where Christian control was stronger: Alcira (especially in May), Cullera (especially in April through August), Liria (especially July–August), Peñíscola (especially December), Castellón de Burriana, Murviedro (especially July), Onda, Játiva again, Uxó, Jérica (some forty items, especially January through March), Beniopa-Gandía, Segorbe, and a scattering of southern places such as Guadalest, Ebo, Gallinera, and Cocentaina. Thus the impressive jumble of this codex, when given shape and order, diminishes to a much more modest movement of population, mostly in 1248, and to a more modest displacement of Muslims except at Játiva and one or two localities. Jaume himself records that Muslim numbers and morale in the field were still high, and the fighting continued bitterly into the next year. Other data may modify these impressions, but it seems unwise to see more in the *Repartiment* itself than a serious effort to increase Christian settlement, mostly in the south below the Júcar River.

The summary analysis above has stressed time patterns and the annual comparative pacing of the grants. Ramón Ferrer Navarro meticulously calculated the southernmost grants and schematized them by number of proprietors, by type and extent of holdings in each place, by number of houses granted in each, and by type of settler (social origins, class). He concluded that, in 1248–1249 in the south, "a numerous group of Christian settlers" had arrived to offset the rebel presence, with many soldiers and small or large communal groups. Catalans and

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Aragonese with Navarrese predominated, with relatively few outsiders. No real ecclesiastical presence and few nobles appeared. It is notable that the largest single number of grantees is at Bocairente (232), with Guadalest next in importance (eighty-two settlers), then Calpe and Cocentaina (sixty-four and sixty-two), Callosa (fifty-five), Altea and Alcoy (each forty), Jalón (thirty-seven), Laguart (twenty-four), Perpunchent and Finestrat (each twenty), and the others down from eight through seven. The Alcoy mountain area thus received 377 grants (with 232 of these in Bocairente), the Pego valleys sixty-nine, and the La Marina district 329–out of a total of 788.[23]

Like previous analyses, the framework and interpretation here must rest on the assumption that these grants represent straightforward resettlement by a random population rather than a selected group; that each claimant took up his grant at this moment (indeed, that the king intended the grants to intrude settlement in 1248–1249 rather than to elicit recruits or to reward by anticipation his military efforts here or elsewhere); that these 788 grants involved personal residence and had a deterring effect on the many thousands of Muslims still in the area; that the grants involved active displacement of Muslims according to a strategy of immigrating rather than a filling of vacancies left by a minority of Muslims abandoning their properties to remove to Islamic or al-Azraq-controlled lands; and that we have in this codex not a stray survival probably matched by some lost codices but a unique unit responding to the special situation of 1248. These are reasonable enough assumptions, but they need to be explicitly recognized as such, and the conclusions therefore accepted only as tentative and somewhat conjectural.

The remarkable series of crown registers, with their thousands of documents for Jaume's reign, do not begin until August, 1257, when the al-Azraq war had entered its final phase. A half-dozen charters must fill in the gap. The best known of these is an arbitration by the queen and a committee of "wise men," between the king and Prince Pedro of Portugal, who held a grand fief in northern Valencia—the Segorbe, Almenara, Murviedro, and Castellón districts. It concerns "the expulsion of the Saracens" and "the settlement of those places." It dictates that, "while the present war of the Saracens in the places around Murviedro, Segorbe and Almenara shall last," the king is obligated to pay for protecting and garrisoning "the frontiers [*fronterias*] of the aforesaid castles," putting forty-five men into Murviedro, thirty-five into Almenara, thirty-five into Segorbe, and five into Castellón, all drawn from the prince's household or at his choosing. In return, the prince must accept settlements the king has already made in these places but can modify or approve the charters involved. The ambiguous date of this arbitration is more likely February of 1249 than 1248; the latter would hardly allow time for Jaume to have put his expulsion into practice or to have introduced already new Christian settlements into those districts, much less allow time for the stages of a

[23] Ferrer Navarro above in n. 14. In studying the *Repartiment*, it is important to note which items were not deleted (the Cabanes edition marks deletions, the Ferrando edition supplies the facsimile texts); the notarial deletion indicated issuance of a charter and thus an active grant.

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dispute leading to arbitration. It reveals the war as widespread in the north, even close to Valencia city and in a region previously well consolidated under Christian control.[24]

A Mudejar constitution given to Uxó Valley in August of 1250 grants generous privileges and an amnesty but confiscates the properties "of all who left the valley and were not besieged by them [the Uxó Moors] in the said castle" of Uxó. It also allows these Muslims to travel freely in the kingdom except "to a place or places of the war, nor can they give any provisions of any kind" to the enemy. This constitution indicates that a stronghold close to the coast and Christian control was not under that control until late 1250. It also suggests that a body of Muslim residents had been besieged in the castle and had since fled, the besiegers apparently other Muslims (although, just possibly, in this tortured translation of the original Arabic into Aragonese, Christians). Such merely random survivals suggest in turn a much wider situation of confused guerrilla warfare, a kingdom out of control.[25]

A BODY OF NEW DOCUMENTS IS BADLY NEEDED to give further context and continuity. Among the methods most central to the historian's task is the search for and discovery of these treasures. An area of investigation overlooked to date is papal participation in the al-Azraq war. I have discovered and present here a series of papal documents from the bulls in Jaume's crown archives. Why this rich source remained unknown to Zurita, Tourtoulon, Miret, Soldevila, and other great scholars who treated al-Azraq, is a mystery. This is particularly puzzling in the case of contemporary historians, since F. J. Miquel Rosell in 1948 published a catalog of abstract-titles for the entire collection of nearly 900 bulls.[26] Perhaps they carelessly believed that the bulls preserved in the crown's archives would concern matters narrowly ecclesiastical. One historian did advert to the collection. José Goñi Gaztambide, in his massive history of all the crusade bulls for Spain through the sixteenth century, listed five of the series and rightly titled their effect a "crusade." His one-page summary misdated the crusade as formally active in early 1247, however, and included material dubiously relevant to that decade. He cited the transcriptions of Fidel Fita concerning the provincial councils of 1248–1250, which gave financial support to the war.[27] In any case, Goñi Gaztambide's obscure

[24] Archivo de la corona de Aragón (hereafter, ACA), Cancillería, Jaime I, perg. 1,146. This well-known document is transcribed in Antonio Chabret, *Historia de Sagunto*, 2 vols. (Barcelona, 1888), 2: 401–02; and more accessibly in Fidel Fita, "Concilios tarraconenses en 1248, 1249 y 1250," *Boletín de la Real Academia de la historia*, 40 (1902): 444–58. The transcription has nearly two dozen small errors, not substantial but irritating; I am editing it afresh for publication in *Sharq al-Andalus*.

[25] Huici, *Colección diplomática*, doc. 383; Cabanes, *Documentos*, doc. 547 (August 1250, a very late translation of the lost Arabic original). In August of the previous year, the boundaries of Alcira had been clarified to include Cullera, Corbera, Alfandech, and other places (doc. 506).

[26] Francisco J. Miquel Rosell, *Regesta de letras pontificias del Archivo de la corona de Aragón* (Madrid, 1948).

[27] José Goñi Gaztambide, *Historia de la Bula de la cruzada en España* (Vitoria, 1958), 185. Fita is above in n. 24. Emilio Morera Llauradó, *Tarragona cristiana: Historia del arzobispado de Tarragona y del territorio de su provincia (Cataluña la Nueva)*, 2 vols. (Tarragona, 1889–99; rpt. edn., 1981–82), the standard

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notice seems not to have entered the awareness of historians concerned with these troubles. Each of the papal series needs to be given context and related to the others and to the war at its pertinent state. The character and scope of the al-Azraq war will then be evident.

The bulls run from October of 1247 to March, 1253, precisely those critical early years for which no registers and very few random documents exist. A full transcription has been prepared for publication. The first in this series may serve as background. King Jaume had sent an envoy with written petitions to Pope Innocent IV, to which the pope returned an answer on 18 October 1247. Innocent noted that he "conceded some of your petitions, effectively and gladly," but "others for good reason we decided should not be granted." The pope then soothed the king's feelings, conveying his special attachment for Jaume. The royal embassy would have gone to Rome too early to have included mention of al-Azraq's first act of revolt. The Muslim's truce or treaty of 1245 still had the better part of a year to run. Valencia seemed quiescent. The king had spent May, June, and half of July in that kingdom at the capital city before returning to Aragon; the *Repartiment* grants for 1247 roughly coincide with that visit, though not exclusively. In April, the pope had transferred the disputed Segorbe region of Valencia to the Castilian-dominated diocese of Albarracín beyond Valencia's border. In short, the letter testifies both to the easy communication between Jaume and Innocent and to the tranquillity of conquered Valencia when the embassy left in early fall.[28]

Al-Azraq seems to have made his move around this time. Two weeks after the pope penned this document, King Jaume was in Valencia, where his charter datelines show him moving about for nearly two full years (31 October 1247 to 13 August 1249). He failed to bring the rebels to heel by his presence or diplomacy, and, on 1 December 1247, Jaume held his famous parliament or council deciding for general expulsion. Datelines show Jaume besieging the rebel Muslims at Luchente through most of March, 1248, followed by his massive settlements in and around Játiva from March through July. Those same months of spring and summer show a high tide of *Repartiment* entries throughout the south; most of this general flow may represent encouragement intended to stimulate the troops and war financiers rather than actual possession yet. Both the Luchente siege and references to "while the war of al-Azraq lasts" indicate the intensity of the military campaign.[29] What is surprising, however, is the entry of Rome into the conflict now.

On 12 November 1248, a year after Innocent's first peaceful communication to Jaume, he dispatched an encyclical to the archbishop of the metropolitanate covering Jaume's sub-Pyrenean realms, including as well all bishops, abbots, priors,

history, has nothing useful on the revolt. Following Zurita's chronology, it has Jaume first gather his army in 1254 to strike back (2: 73–74).

[28] ACA, Bulas pontificias, Innocent IV, leg. 9, no. 32 (14 October 1247): "regia serenitas non miretur si alias [petitiones tuas] ex iusta causa non duximus admittendas."

[29] Miret i Sans, *Itinerari*, 190–98, Luchente siege on 190, 191, 195. *Repartiment*, no. 2539: "quantum durabit guerra de Alazrac" (29 February 1248, leap year; or 28 February 1249 if the calendar is Incarnational).

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archdeacons, deans, "and other prelates of churches," also chapters, religious houses, "and other ecclesiastical persons throughout the realms, counties, lands, and regions" of the king. The pope recalled Jaume's unique status as successful warrior "against those who blaspheme against Christ," and his "anxious labors and innumerable expenses" for so long a time. The church must help the king "against a people ignorant of God" who "have arrogantly rebelled against the king." Without "difficulty and delay," these variegated churchmen are to supply "suitable aid for so necessary and reasonable a cause," thus winning God's gratitude and the pope's.[30]

Such documents do not come out of the blue. King Jaume had experienced a shocking "great war," as his memoirs reveal; by at least late summer, he must have recognized the inadequacy of his resources to cope with it. He had won a previous alleviation from Rome for his Majorcan settlers that March, a dispensation to engage in the otherwise restricted trade with inimical Muslims, so as to attract more settlement to the island.[31] In similar vein, he needed some financial arrangement to open the coffers of his dioceses and monasteries from end to end of his realms. Jaume did not stop there; he also asked Rome for a crusade. These requests for Roman intervention must have been painful for him, a bitter moment in his hitherto triumphant career, revealing to Christendom as they did the dimensions of his military plight.

Jaume's metropolitan at Tarragona had obviously tried to apply church revenues to the king's need even at the start of the war. The Knights Hospitaller had refused to contribute and had carried their protest to Rome. On 6 April 1248, Innocent reminded the archbishop of the special immunity enjoyed by Hospitallers for their Near Eastern work and ordered him to cease "molesting" them. A reference to any previous papal concessions "notwithstanding" may simply be a formula for juridical completeness, since no such letters seem yet to have gone to Tarragona. On 28 March 1249, Innocent again intervened with a more elaborate letter on the same Hospitaller situation, this time telling the abbot of Poblet that neither the archbishop nor his suffragan prelates could continue contemptuously (*temere*) to demand contributions from the knights.[32]

Innocent followed his financial appeal with a crusade circular on November 13th "to all Christ's faithful." The pope recounted that Jaume had informed him how "the infidel people still walking in darkness" and slavery, but unceasingly persecuting those who enjoy Christ's freedom, "have revolted against him powerfully and fiercely." Innocent therefore called for "many to gird themselves through the glory of the cross to make a chivalric body as an army of Jesus Christ."

[30] ACA, Bulas pontificias, Innocent IV, leg. 10, no. 43 (12 November 1248): "labores sustinendo anxios et sumptus innumeros subeundo . . . incessanter." "Blaspheme" can bear the sense of "reject as false," as well as meanings of abuse or blasphemy.

[31] Archivio Segreto Vaticano (Rome), Innocent IV, Reg. Vat. 23, fol. 516v, ep. 682 (21 March 1248): "plenius reddi populosa valeat," an extension or confirmation of privilege from Gregory IX.

[32] J. Delaville le Roulx, ed., *Cartulaire général de l'ordre des hospitaliers de S. Jean de Jérusalem (1100–1310)*, 4 vols. (Paris, 1894–1901), 2: doc. 2,467 (6 April 1248); and doc. 2,495 (28 March 1249). This dispute, and its suggestion of dissent by similar orders, has not been noticed. The contributions are the Holy Land "twentieths" eventually transferred by Rome to King Jaume.

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For all who, with repentance and confession, "take up the sign of the living cross, to give that king assistance against those people" by personal service in arms, the pope conferred "full pardon of their sins." Those who merely gave material support, "according to their own capacities," also received "that indulgence which was granted by the general council [at Lyons in 1245] to those sending support to the Holy Land."[33]

Yet another letter went out from Rome that day, "to all the collectors of the twentieth of ecclesiastics for support of the Holy Land," who had been commissioned in Jaume's "realms, counties, lands and district." It covered much the same ground: the king's special services to Christendom against the Muslims and the order sent from Rome to all the king's prelates and ecclesiastics to provide timely financing against the Valencian rebels "so that with divine cooperation and their help he could triumph." This time, the pope mandated that these tax-collectors for the Holy Land one-twentieth not require it from Jaume's ecclesiastics for a three-year period.[34]

Innocent's bull elevated the al-Azraq war into a crusade for Christendom, with the usual trappings—the ritual of putting on the cross, the Holy Land indulgence proclaimed by the recent ecumenical council, and the transfer of the crusade "tithe" to support this needier front. No details in this document survive as to its preaching (such as exist for the Valencian rebellion of 1276), although the legatine commission and the mechanism for selecting the popular preachers can be seen in a subsequent papal letter. As to foreign participation, Jaume himself announced his expectation of a great rallying by "the men from our realm[s] and from other lands"—doubtless a postwar recollection of such help, placed in his memoirs about the war's beginning to vindicate his short-sighted policy of expulsion. Foreigners were not needed to make a crusade, but they had come to the original conquest and probably came again now.[35] At this moment, of course, the Castilians under St. Fernando III were locked in their own great siege against Seville, about to fall.

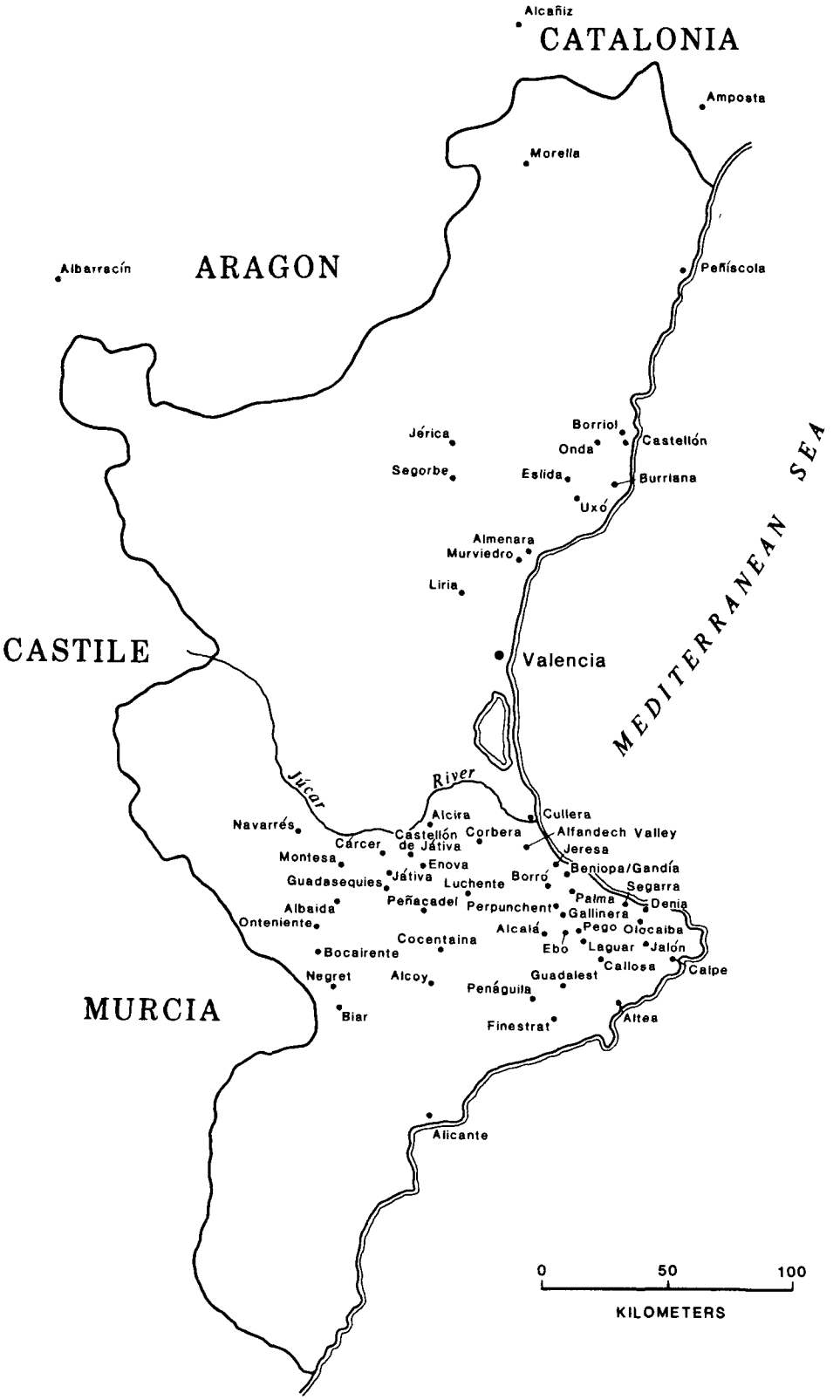
The documents discussed above, from Jaume's papal series, have no echo in the great collections of the Vatican. At this point, however, two versions of a bull converge: a copy in the Roman registers and the original, which lodged in Jaume's archives. The Roman copy was included in the mass publication of Vatican registers but was never noticed by historians of Valencia. The crown's original parchment will be used here. The tithe arrangements of November, 1248 apparently encountered some reluctance or procrastination. To curb the malingerers, Innocent put into the Tarragona metropolitan's hand an effective weapon

[33] ACA, Bulas pontificias, Innocent IV, leg. 10, no. 44 (13 November 1248): "infidelium populus, qui adhuc in tenebris ambulans . . . persequi non desistit, contra ipsum insurrexerit potenter et uiriliter in partibus valentinis: nos cupientes multos accingi per crucis gloriam ad corporale tyrocinium exercitus Ihesu Christi, omnibus uere penitentibus . . . qui assumpto uiuifice crucis signaculo ad prestandum ipsi regi contra predictum populum auxilium in propriis personis accesserint, plenam suorum peccatorum ueniam indulgemus"; "iuxta proprias facultates, illam indulgentiam elargimur que mittentibus in Terre Sancte subsidium in generali concilio est concessa."

[34] ACA, Bulas pontificias, Innocent IV, leg. 10, no. 45 (13 November 1248): "uicesimam ecclesiasticorum prouentuum Terre Sancte deputatam . . . ad triennium non exigatis."

[35] Jaume I, *Llibre dels feyts*, chap. 365: "los hòmens del regne nostre e de les altres terres . . . hi venrà." Jaume several times adverted to the body of foreigners who came to the original Valencian crusade.

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MAP: The Thirteenth-Century Kingdom of Valencia: Places Mentioned. The larger modern borders to the south and west are the ones shown here.

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on 25 January 1249. "Because perhaps there are some who might wish to disengage from so praiseworthy a project," the pope "concedes the power of compelling if necessary by ecclesiastical censure" such ecclesiastics, with proper canonical warning at first and no appeal allowed at the end, "to give up this twentieth fully and openly." The letter also reveals that the prelates of the realm had met in council and voted the Holy Land twentieth from every corner of the realms, "for one year, to the said king in support against the Saracens." Even exempt religious orders, churches, or houses fell under the demand for contributions.[36]

Meanwhile, incidental documents reveal glimpses of the war. On 13 December 1248, the king removed export fees on the passage of Muslims from the city and kingdom of Valencia. This privilege was "in perpetuity" and would seem to apply to the slave trade, newly refreshed by the war, rather than to the passage of emigrants or pilgrims—who are later documented as subject to a fee.[37] The arbitration agreement between the king and Pedro of Portugal in February of 1249, noted above, reveals the extent of the war in the north. Throughout 1249, the *Repartiment* shows a pattern less active than for 1248 and less oriented toward the south. Murviedro, Castellón, Onda, Liria, Uxó, Jérica, Alcira, Peñíscola, and Cullera are prominent recipients of grants. Játiva continues with only a mild pattern of immigration from January through July. Alcira's steady trickle runs from January through June, Corbera's from April through August. Since each area is a region of some size, not merely a town, we should not be too prompt to see a large displacement of Muslims. It was in August, 1249, by Momblanch's conjectural reconstruction, that the pitched battle and Christian defeat took place in northern Valencia, and al-Azraq's seizure of strategic Peñacadel threatened Christian communications in much of the south.[38] The king's datelines show him in the Valencian kingdom all year until mid-September.

EVEN THOUGH NO FURTHER PAPAL MESSAGES SURVIVE from this year, a significant ecclesiastical charter from a separate collection of Jaume's parchments does bring the wider crusade efforts into focus again. This document was cited by Tourtoulon in the nineteenth century, and his reference noticed by one or another later historian. Fita published its transcription in his account of the Tarragona provincial councils. The charter should be read in the context of our papal series. It constitutes an agreement reached at a provincial council convoked by the metropolitan of Tarragona and personally signed by the major prelates. It notes a previous council at Tortosa "this year," at which they had voted the king the crusade-twentieth of the province's ecclesiastical revenues.

[36] ACA, Bulas pontificias, Innocent IV, leg. 10, no. 157 (25 January 1249): "a tam laudabili proposito"; "in subsidium contra Sarracenos integre." The registered version in Arch. Sec. Vat., Innocent IV, Reg. Vat. 6, fol. 22, no. 317, is not published but only noticed by Elie Berger, ed., *Les Registres d'Innocent IV, publiées ou analysées d'après les manuscrits originaux du Vatican*, 4 vols. (Paris, 1884–1921), 2: 37, no. 4309.

[37] Huici, *Colección diplomática*, doc. 340; Cabanes, *Documentos*, doc. 481 (13 December 1248).

[38] Momblanch, "Alazrach," 17–18; *Al-Azraq*, 77–80.

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Problems with the calendar make the Tortosa date ambiguous. The Tarragona provincial council of 22 April 1247 had voted to change the opening date of their annual council from the fourth Sunday of Easter to the fourth Sunday of Lent, but adjustments could of course be made for crises, alignment with royal councils, and the like. The meeting at Tarragona, dated 15 March 1248, must have followed the Florentine incarnational calendar, since the Valencian prelate had taken office eight months after March, 1248. Thus the Tarragona meeting took place in our March, 1249, but the document's reference to "this year" could mean any time from late March, 1248 to early March, 1249 for the Tortosa meeting. Presumably, that earlier council took place soon after the pope's instructions of mid-November, 1248 and at least before his admonition against malingerers on 25 January 1249.

At the meeting at Tarragona in 1249, the prelates voted the twentieth again for the coming year "in support of the business noted." They describe this business as: "Having before [his] eyes zeal for the Catholic faith, [Jaume] with careful solicitude labors for the expulsion of the Saracens from the kingdom of Valencia." They note that the king's "need," their "own utility," and the pope's "will and consent" converge as motives. The term for the first year's collection was to end on the coming 31 May 1249, with the second year now voted beginning from that date. This fiscal-year calendar means that the king's first year of collection was closer to a six or seven-month period at best. Presumably, the triennium mandated by the pope would have occasioned a third vote by the prelates for the term 1 June 1250 to 1 June 1251.[39]

The signers deserve a glance, as a remarkable assemblage from all the king's realms, a testimony to the universality of his clergy's concern. After the signature of the Cistercian archbishop of Tarragona over all the realms, Pedro de Albalat (Catalan Pere d'Albalat, 1239–1251 in office), comes the elderly bishop of Tortosa on Valencia's northern border, Ponce de Torrella (Catalan Ponç, 1213–1254); his old man's wavering letters close with a bizarre form—"subsubscribo." Vidal de Canellas (1238–1252), still celebrated today as a Roman Law jurist, signed for his Aragonese see of Huesca. Pedro de Centelles (Catalan Pere, 1241–1252) signed for Barcelona and the Dominican Guillem de Barberá (1248–1255) for Lérida. For troubled Valencia itself, the great Dominican Andrés de Albalat (Catalan Andreu d'Albalat, 1248–1276) closed the list of witnesses with a neat, personal script. The notary supervising the draft as agent of the archbishop was Guillem d'Alaric, a public figure in his own right. Delegates important enough to be included as active parties forming this contract were the Barcelona canon and jurist Pere Albert, as famous today for his Romanist influence on Catalan law as Canellas is for Aragonese law. Albert served as procurator for the understandably absent Berenguer de Castellbisbal, bishop of Gerona (1245–1254), whose tongue had

[39] ACA, Cancillería, Jaime I, perg. 1,150 (15 March [1248] 1249). Fita's version was transcribed for him by the archivist Francisco de Bofarull ("Concilios," 445n. 5), and is confusingly inserted as though part of the council's acts (clarified only by note 9 on 446; the charter is on 446–47). I have reedited it in more complete form, with the document above in n. 24. I do not find Morera's *Tarragona cristiana* or *El Archiepiscopologio de Luis Pons de Icart*, ed. José Sánchez Real (Tarragona, 1954), very useful on these councils.

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recently been cut out at the king's command in a brutal episode that brought papal thunders and penances down on the royal head.

Berenguer Bord served as procurator for the bishop of Vich Bernardo or Bernat de Mur (1243–1264), Benito or Catalan Benet de Prats as procurator for Bishop Ponç de Vilamalur of Urgell (1230–1255), Geraldo the cathedral sacristan for his bishop of Zaragoza recently moved from the Valencian see, Arnaldo de Peralta (1248–1271), and Sancho Pérez for the Tarazona bishop García Frontín (1219–1254). No one represented Bishop Pedro Ximénez of Pamplona, busy at the time in major controversies with the king of Navarre. For the cathedral chapter of Barcelona, Pere Albert served; for the Lérida chapter, its chanter Pere de Lerato; and, for the Huesca chapter, the canon Sancho de Boleya. Two great Benedictine abbeys represented the orders: Dalmau de Çagarriga (1235–1256) as the abbot of Ripoll, and Pere d'Amenys (1232–1255) as abbot of Sant Cugat del Vallès. These men were not meant to constitute an exhaustive list of clergy but were widely representative of the churches of Aragon and Catalonia. Majorca was absent, as under the direct jurisdiction of the pope and doubtless also by reason of its own troubles.[40]

A final charter for 1249, hitherto overlooked, shows a glimpse of the baronial armies gathering around the king. It was drawn up at Valencia on 1 April by Jaspert IV, viscount of Castellnou, a great and venerable fief in Pyrenean Catalonia and Roussillon. The document directly concerns the return of "pledges" by the king for debts or loans involving Jaspert and his brother Dalmau I, lord of Sureda, "from the first day on which we came to you [the king] in the kingdom of Valencia up to the fifth day from the close of the next coming month of October." As part of this arrangement, "we promise to serve you with thirty-six animals up to the aforesaid day." To cover expenses, "by reason of the service in the aforesaid time, we receive from you the revenues of Collioure from the next coming feast of St. John the Baptist, for one year." (Collioure is a Mediterranean port still important in the commerce of southern France.) Both brothers signed. Since the feasts of the Baptist are 24 June (birth) and 29 August (martyrdom), these troops anticipated an initial service from the summer of 1249 to the summer of 1250.[41]

For the year 1250 itself, very little relevant documentation remains, aside from the papal series under discussion here. Settlement charters at Onda (April) and perhaps at Borriol, Morella, Uxó, and Peñíscola (1250–1251) may suggest that the settlement movement and some displacement of Muslims was continuing. The amnesty and agreement with the Muslims of Uxó especially indicate a state of pacification in Uxó by August. Phrases such as "at the time we expelled the Saracens from the realm of Valencia" may refer to a major consolidation or may merely be stock phrases referring back to the original conquest in 1232–1245. In August, 1250, King Jaume conferred on Játiva definitive confirmation of all its

[40] For Majorca's inability to attend, see n. 57 below and text.

[41] ACA, Cancillería, Jaime I, perg. 1,151 (1 April 1249): "racione servicii predicti temporis, recipimus a vobis redditus Cauchiliberi . . . in unum annum."

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Christian grants, as well as a constitution establishing its market, settling municipal boundaries and jurisdiction, and "beginning" its settlement by Christians. Taken with Játiva's Mudejar constitution five months (or perhaps seventeen months) later, and with the extensive grants of 1248, this activity signals consolidation and an advanced state of control there. The *Repartiment* is now no longer very helpful but Miret's datelines show that the king continued in the Valencian kingdom, mostly at Morella and Valencia city, through the first five months.[42] Since the king did not return through most of 1250, all of 1251, all except three months of 1252, all of 1253, and (except for two brief visits) all of 1254 through 1257, it might appear that the war had ended. The crisis had certainly passed, and some sort of stalemate may have prevailed as Jaume turned to neglected other affairs. One major affliction of a domestic nature is well known to historians: Jaume had convoked a parliament or council at Alcañiz in February of 1250 in order to submit to arbitration the quarrels and bitterness over his recent decision on the future division of his realms among his heirs. But this domestic and baronial infighting was only one of the king's troubles, perhaps rather a distraction from the more serious problem of Muslims and their Christian helpers.[43]

Five papal letters of March, 1250 throw startling light on these apparently quiescent years. On 18 March, the pope ordered the Templar master in Catalonia and Aragon, and the head of Hospitallers at Amposta, to help King Jaume.[44] "The said king, as a Catholic prince, perseveringly strives to increase the faith, tirelessly battling the Saracens," and has asked Rome for help. Innocent consequently ordered "that you give counsel and timely aid to the king against the aforesaid Saracens, and the Christians favoring them in this."[45] Who are these Christians?

[42] Miret i Sans, *Itinerari*, 201–06.

[43] Cabanes, *Documentos*, doc. 518 (29 January 1250 or 1251), a general confirmation of Christian holdings that "erant sarracenorum Peniscole tempore quo sarracenos expulimus a regno Valencie," with the settlers in turn promising to sell any properties elsewhere and not to sell their Peñíscola properties for the usual ten-year period after a grant. The second provision seems to indicate that both expulsion and resettlement were recent. "Expulsion," however, can include transfer of title, with Muslim farmers reduced to rental tenants, and especially removal from the city or into a section of it. Doc. 519 (12 February 1250 or 1251) similarly conveys the small town of Borriol to a group licensed to bring in settlers, with the same ten-year promise; although it was in the northern area of Valencia, Borriol remained a mix of Christians and Muslims down to Morisco days, a circumstance calling for caution in judging these documents apparently replacing populations. Doc. 520 (16 February 1250) confirms the property assignments given by the previous lord of Morella and may have no reference to settlement strategies prompted by al-Azraq's revolt. Doc. 530 (29 April 1250) confirms to the Christian settlers of Onda the town boundaries and territory "prout sarraceni eiusdem loci sarracenorum tempore eos melius habuerunt"; the phrase probably harks back to pre-crusade times, but the timing of the document may be significant. Doc. 548 (3 September 1250) is a general license for the Hospitallers to bring in settlers at Cullera below Valencia: "stabiliendi et populandi ad octavum omnes alquerias et terminum castri." Doc. 579 (8 September 1251) allows removal and resettlement of Castellón de Burriana onto the flatlands. Doc. 587 (23 January 1252) is the Mudejar constitution for (presumably reconstituted) Játiva. On the tangled question of the stages of Játiva's "fall" or assimilation to full Christian control, see my *Islam under the Crusaders*, 342–44, with works cited. Míkel de Epalza's *Xàtiva musulmana (segles VIII–XIII)* (Alicante, 1987) also touches on the problem.

[44] Delaville le Roulx, *Cartulaire*, 2: doc. 2,517 (18 March 1250), from the ACA, Bulas pontificias, Innocent IV, leg. 11, no. 50. The Miquel Rosell catalog (no. 161) does not indicate this as published. The charter is 27.5 cm. by 23 (folded to 20.5), the seal now missing; scribal name Arm. Parm. on the fold.

[45] ACA, Bulas pontificias, Innocent IV, leg. 11, no. 50: "dictus rex, tanquam princeps catholicus, ad augmentum ipsius fidei perseueranter intendat impugnando assidue Saracenos"; "quatinus eidem regi

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They can hardly be the forces of the new king of Castile, Alfonso X the Learned; those troubles are a few years in the future. That same day, Pope Innocent wrote also to Jaume, elaborating on the special place the king held in his heart and on papal resolve to see that the kingdom came to no harm. "From the tenor of your letters," however, the pope understood that "many evils [*gravamina*] are visited on you and your kingdom" by some people. Saying that he was moved and sorrowful, the pope assured Jaume that Rome will not allow the king and kingdom "to be molested unduly by anyone," insofar as could be helped. Letters had also gone from Jaume's Queen Violant to Pope Innocent. Still on that same day, Innocent returned to the queen a similarly consoling promise to help against the "many afflictions" besetting the kingdom.[46]

The most notable occurrence of that year is signaled by a message in Arabic from al-Azraq at his headquarters and stronghold of Alcalá to Queen Violant. In his letter, al-Azraq refers to King Jaume as "sultan, king of the Christian lands and king of eastern al-Andalus," thus acknowledging Jaume's rule over all the Valencian regions. Al-Azraq had received the queen's envoy Joan de Mur, who had presented a letter from King Jaume. The Muslim has read it, he wrote, and appreciated the contents. Al-Azraq sent the queen an embassy of his own; the principals were his cousin Abū 'l-Ḥasan, the *qā'id* Abū 'Umar, and, as head, his confidant, Abū 'l-Qāsim. Al-Azraq requested safeguard for this embassy in going and returning. Abū 'l-Qāsim was to speak for his master, as he was fully briefed. The queen in turn was assured that she could confide in him as if to al-Azraq. The tone of this discreet and therefore obscure message is courteous and deprecatory (perhaps only as a matter of form), and the author expects something good from the encounter, "if God wills." Jaume had been in Valencia city through February and was still there on the date of al-Azraq's letter in March. Since the king shared his projects and diplomacy with Queen Violant, it is no surprise to see her taking a central role in the correspondence. Al-Azraq's tone of diplomatic amity also conveys his sense of security. The king has made the first move, after all, and al-Azraq will exchange envoys with him on equal terms. The convention of accepting a Christian ruler as having some extrinsic claims over one's resources or territory was a ploy common enough in frontier warfare and rationalized in Islamic terms. The military phase of the war seems to have waned and a diplomatic phase begun, albeit a phase during which al-Azraq required a safeguard for his envoys.[47]

The pope's continuing concern for the beleaguered king becomes more explicit in a long letter of March, 1250. Directed to the metropolitan of Tarragona and "to the bishops established throughout the realm of Aragon, Valencia, and Catalonia," it said that the pope felt "stabbed through by a spear" to realize how the Muslims were "persecuting" the faith relentlessly, laboring "with their usual persecution in cruelty to extinguish it" and to eliminate "its worshipers from the

contra Saracenos predictos et Christianos in hoc fauentes eisdem . . . detis consilium et auxilium oportunum."

[46] ACA, Bulas pontificias, Innocent IV, leg. 11, nos. 50–51 (18 March 1250): "ex tenore litterarum tuarum accepimus tibi et regno tuo a nonnullis multa grauamina inferuntur."

[47] Alarcón and García, *Documentos*, and Barceló, *Documentos*, above in n. 12.

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face of the earth." Against this arrogance, the Christian people must rise, countering the Muslim attack and promoting the faith. Consequently, "since the Christian people in the regions of Spain are under attack by the frequent outrages of the Saracens," Christians must resist boldly and perseveringly. King Jaume "battles with tireless efforts in those regions against the aforesaid hosts, not sparing his person or resources, an athlete for protecting the orthodox faith and for extending Christian worship." The pope ordered the metropolitan to preach the crusade "both by yourself and through other prudent and decent secular or religious clerics, whom you know to be suitable for this," in order to "rouse with frequent exhortations all the faithful throughout Catalonia and the regions of Spain under the jurisdiction of the said king" to battle against those hosts "alongside the king." By the authority of God and of Peter and Paul, Innocent conferred "that indulgence and that privilege, which are granted to those rescuing the Holy Land," on all the king's subjects "who personally go with the king" against these enemies, and those who "lead suitable warriors at their own expense," as also those who go at another's expense for one year or who give the king "proper [material] support from their goods."[48]

As late as March, 1250, Pope Innocent was reiterating his crusade, therefore, offering the crusade indulgence, and designating the archbishop as papal legate for its preaching and for selecting a corps of preachers. The language is stronger than in his previous letters and his witness clear as to the continuing danger from al-Azraq. Innocent's bull was dispatched to Jaume only a week after al-Azraq had announced his reception of the king's letter and had prepared his embassy to Jaume and Violant, meaning that Jaume had opened negotiations with his enemy at the very time he was seeking renewed crusade status and military aid from the pope. The letters of pope and Muslim rebel converge to indicate not a true stalemate but a turning point in this crusade.

The pope's letter of 18 March to the Templars and Hospitallers, seen above, had raised the problem of Jaume's having to resist not only the Muslims but "the Christians favoring them" in their war, and another bull ten days later took up the matter directly.[49] Innocent professed himself "troubled with deep sorrow" to hear of an "evil example" by Christians. Jaume sent word to him that "a number of

[48] ACA, Bulas pontificias, Innocent IV, leg. 11, no. 54 (22 March 1250): "venerabilibus fratribus . . . per regnum Aragonie, Valentie, et Cathalonie constitutis . . . tristitie telo confodimur"; "fidem ipsam odio continuo persequentes, eam extinguere solita crudelitatis persecutione laborant"; "ipsius cultores abolere de superficie terre"; "cum populus christianus in Yspanie partibus frequentibus Sarracenorum insultibus impetatur"; "rex illustris Aragonum pro fide orthodoxa tuenda et cultu christianis nominis ampliando, uelud eiusdem fidei specialis adleta nec persone parcens nec rebus contra prefatos hostes in eisdem partibus indefessa strenuitate decertet"; "tam per uos quam per alios seculares uel religiosos clericos prouidos et honestos, quos ad hoc ydoneos esse cognoueritis uniuersos fideles per Cathaloniam et partes Yspanie iurisdictionis dicti regis constitutos . . . una cum memorato rege . . . potenter exurgant"; "illam indulgentiam idemque priuilegium elargimur que Terre Sancte subvenientibus conceduntur"; "qui cum predicto rege personaliter . . . accesserint, quique iuxta facultatem et qualitatem suam bellatores ydoneos in expensis propriis duxerint destinandos"; "uel de bonis suis eidem regi congruum propter hoc subsidium."

[49] ACA, Bulas pontificias, Innocent IV, leg. 11, no. 55 (28 March 1250): "nonnulli Christiani tam nobiles quam alii uestrarum ciuitatum vel diocesarum contra eum Sarracenis consilium et auxilium . . . exhibere presumant."

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Christians [*nonnulli*], both nobles and others of your cities or dioceses dare to offer counsel and aid to the Saracens, against him, to the damnation of their souls." Innocent "warns, requests, and seriously exhorts" the prelates of Spain, first to order the malefactors to desist from this outrage, and then to "lay upon them a sentence of excommunication, having it solemnly proclaimed on Sundays and feast days in places you think suitable" until the evildoers make "appropriate satisfaction." The pope directed his letter "to the archbishops and bishops constituted throughout Spain and Catalonia," although the complainant was Jaume, and the problem concerned Jaume's realm. This action may also suggest Castilian interference or manipulation of the al-Azraq war early and on a large scale. It suggests too that the Catalan bishops, addressed in tandem with the rest of Spain, had someone to admonish, perhaps the baronage who had resisted Jaume's project from its very beginnings at the Valencian council of 1247. The letter of 18 March to the military orders, precisely those bodies "in Catalonia and Aragon," reinforces this impression of a major domestic disloyalty.

THE SERIES OF PAPAL DOCUMENTS ON AL-AZRAQ'S WAR breaks off with this letter of 28 March 1250. The few references from Rome during 1251 mostly reflect the domestic concerns that kept the king away from Valencia nearly all that year. In March, 1251, Innocent took occasion, during negotiations for proper distribution of Jaume's realms to his heirs, to witness that "Valencia is far greater in revenues and profits than the County of Barcelona," an assessment significant in its context of arbitrating a fair division. Jaume would have been reluctant to let any section of so rich a prize slip into the hands of an independent Muslim chief.[50]

A letter concerned directly with Valencia, and related to the dislocations of war and to the opportunities of the region, issued from the pope's chancery at the very end of the year, on 23 December. The pope sharply rebuked the barons, ecclesiastics, and the king himself for bringing Muslim settlers into the Valencian kingdom. Innocent realized that economic motives had to be served, since the letter ultimately condoned the practice insofar as the bishop of Valencia would monitor and approve it. "Certain barons and exempt religious, and also others who hold castles and towns as well as other possessions, have brought in and are bringing in Saracens to settle at many places of the said realm, contrary to the vow made by the said king and to the aforesaid excommunication, to the peril of their souls and the serious detriment of the church in Valencia—the king himself doing the very same thing in some places." This general decree, coming at the end of two years of settlement grants to Christians, raises the question as to how many immigrant Christians became landlords over Muslim-tenanted properties rather than simply displacing Muslims. The picture certainly does not support the thesis of large-scale displacement of Muslims by Christians. Whatever expulsion took place probably consisted of removal of Muslims from some cities (either to a single

[50] ACA, Bulas pontificias, Innocent IV, leg. 12, no. 63 (Lyons, 5 March 1251): "et postmodum rex ipse regnum Valentie adquisierit, quod in redditibus et proventibus comitatum ba[r]chinonensem multum excedit." The Arch. Sec. Vat. registered version, Reg. Vat. 22, fol. 88v, curiales no. 46, was published by Berger, *Registres d'Innocent IV*, 2: 235, no. 5315.

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neighborhood or outside the walls) and of the further migration of elites and governing classes to Islamic lands.[51]

Since there had been no expulsion, the embarrassing problem remained of the king's vow. In that letter of 23 December 1251, the pope told the bishop of Valencia to do what seemed best "about commuting the king's vow" and about "relaxing or strengthening the aforesaid excommunication." This earlier letter clarifies for us the letter Pope Innocent sent to Bishop Andreu of Valencia early the next year, on 1 February 1252. The pope had received the bishop's account about how Jaume had "obliged himself in promises and vows and, before him [the bishop], confesses himself guilty in that he did not render to him what he promised." The ambiguous wording might conceivably apply to other promises, such as endowments. The "vows" and "promises" here are plural, however, and could reasonably include or feature the expulsion that had reversed into an immigration policy and had drawn the pope's ire nine months previously.[52] A companion bull to the king told how "the Valencian bishop Friar Andreu effectively revealed to us what your royal highness committed to him to explain."[53] To help the king and his salvation, papal authority had been given to the bishop to settle the matter. The first letter had left the choice to the bishop "of commuting these promises and vows, and of dispensing from them." Jaume later referred to these vows most solemnly in his deathbed will of 1276, passing the obligation on to his heir. The will reports that Jaume had "promised the supreme pontiff, and sent him our sealed charter, that we would eject the Saracens from our land, and we had previously promised the very same before the altar of Our Lady St. Mary of Valencia, and because the supreme pontiff granted us the tithe for that reason, . . . none of these Saracens should remain."[54]

Settlement related to the al-Azraq troubles may have continued in 1251; fortified Castellón did move from its hillside down to the Burriana plain, and Peñíscola castle was given its Christian charter (unless the ambiguous date is taken as 1250). In January, 1251, or more probably 1252, the Muslims of Játiva received their redrafted privileges and constitutions. The king's datelines show him in Valencia city and Játiva from January through April of 1252. But Jaume was already

[51] Arch. Sec. Vat., Innocent IV, Reg. Vat. 22, fol. 133v, no. 146 (23 December 1251), also published by Berger, *Registres d'Innocent IV*, 3: no. 5,582. The pope adverted to the original conquest and "quibusdam sarracenis . . . ex pacto in regno predicto, suis exigentibus culpis expulsis," in which the expulsion seems to come later, therefore in the al-Azraq period. He reported that the bringing in of Muslims has taken place widely: "in plurima loca eiusdem regni, contra votum dicti regis emissum et predictam sententiam [excommunicationis] . . . ad populandum induxerint et inducant" as a continuing movement. King Jaume himself and even the exempt religious orders, the pope said, were doing the same thing. I have explored this general movement in Valencia, and gathered all the documentation on it, from as early as 1240 into the reign of Jaume's heir Pere, in my "Immigrants from Islam: The Crusaders' Use of Muslims as Settlers in Thirteenth-Century Spain," *AHR*, 80 (1975): 21–42. It is ironic that the movement in Valencia reached its crest, and drew papal thunder, precisely when the Muslims were threatened with "expulsion" by the king's vow.

[52] ACA, Bulas pontificias, Innocent IV, leg. 12, no. 66 (1 February 1252): "commutandi huiusmodi promissiones et uota, et dispensandi super hiis cum rege predicto."

[53] ACA, Bulas pontificias, Innocent IV, leg. 12, no. 65 (1 February 1252): "diligenter nobis aperuit que sibi commisit regalis excellentia expondenda." A letter from the next pope, Alexander IV, on 2 April 1257, noting that Alexander is responding verbally through the bishop of Valencia to the king's embassy, doubtless refers also to the vows (Alexander IV, leg. 14, no. 2).

[54] Tourtoulon transcribes the will in *Jaime I*, 2: 455–60.

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involved in border troubles with Castile's new king in 1252, a situation that became chronic in the several years succeeding.

On 7 March 1253, the pope wrote a long letter praising Jaume to the bishop of Huesca and the abbot of Montearagón. "From his earliest years," the king had shown himself and his kingdom a bulwark of the faith, and had "diligently aimed and carefully worked to propagate it through the neighboring regions of the infidels." Now the king had protested to Rome that "some [people], putting aside the fear of God, presume to molest and disturb the peaceful and tranquil state of his realm." Innocent delegated the two addressees to give warning to the culprits and then to impose "ecclesiastical censure" without the possibility of legal appeal, even if "some persons" held a papal indult protecting them from "interdict, suspension, or excommunication."[55] Was this action aimed at Alfonso of Castile? (The choice of addressees suggests border rather than interior Valencian problems.) Or are these "disturbers" akin to those Christians in Jaume's realms in 1250, seen above, who "favored" al-Azraq's rebels and visited "serious ills" on the kingdom? The latter explanation, or at least domestic dissenters, seems indicated by the choice of the upland bishop and abbot as agents; something more explicit in style, and transcendent in agent, would be indicated if Castile were the target. Pope Innocent, shortly before his death, took Jaume and his family under special protection, in a bull of 17 September 1254.[56]

The impression of stalemate, or even of the war's end, carries through from 1252 to the end of 1256, but this may be more a result of lack of documentation than of the actual situation. *Repartiment* materials continue to tell us little for this period. King Jaume regained from Prince Pedro of Portugal by 30 June 1254 the important towns of Murviedro, Segorbe, Almenara, Castellón, and Morella. The king's datelines show him to have been away from the Valencian kingdom for most of the period after April of 1252. Only in 1258 did he finally settle down for a spring and summer visit, before again abandoning Valencia during 1259 and most of 1260. Jaume had problems elsewhere: the king's sea lanes were under relentless attack. For three years, from 1253 to 1255, a papal communication tells us, the bishop of Majorca was unable to leave his island, offshore of Valencia, because of the scourge of Muslim corsairs.[57] Rome was also worried in 1255 about the great debts burdening the metropolitan of Tarragona.[58] Similarly, in 1257, Pope

[55] ACA, Bulas pontificias, Innocent IV, leg. 13, no. 67 (7 March 1253): "nonnulli . . . molestare ac perturbare presumant"; "contra temeritatem talium . . . efficaciter assistentes."

[56] ACA, Bulas pontificias, Innocent IV, leg. 13, no. 70 (17 September 1254). The Arch. Sec. Vat. registered version, Reg. Vat. 23, no. 217, is published by Berger, *Registres d'Innocent IV*, 3: no. 8046. This is a public privilege with juridical effects against his enemies, not an affectionate gesture.

[57] Arch. Sec. Vat., Alexander IV, Reg. Vat. 24, fol. 76v, no. 531 (18 July 1255): "periculose . . . ad predictam sedem [*Rome*] de tam remotis partibus, medio maris transitu quem pirate frequenter impediunt, pervenitur." Not published in Bourel (see next note) but in Ildefonso Rodriguez de Lama, ed., *La Documentación pontificia de Alejandro IV (1254–1261)* (Rome, 1976), doc. 83. *Ibid.*, doc. 62 (13 May 1255) and doc. 117 (17 October 1255) detail Alfonso's African project. Our papal series from Jaume's archives has a reply from Pope Alexander on 2 June 1255, telling Jaume that he has received the royal embassy and has conceded as much of their requests as was possible (ACA, Bulas pontificias, leg. 14, no. 1). Cabanes, *Documentos*, no. 794, has the transfer of towns by Pedro of Portugal.

[58] Arch. Sec. Vat., Alexander IV, Reg. Vat. 24, fol. 125, no. 1 (21 December 1255): "propter urgentes et necessarias utilitates, oportuit facere magnas sumptus ac per hoc subire gravia onere debitorum."

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Alexander allowed the Valencian diocese to recoup its heavy debts by confiscating for three years the revenues of all vacant benefices.[59] These fiscal difficulties, probably resulting in good part from past contributions to the war against al-Azraq, must have inhibited at this time any costly actions beyond mere containment.

The distraction of the near wars with Castile raised the possibility, as known from past Muslim-Christian interchanges in this region, that al-Azraq would ally with Castile. This southern region had been claimed and almost taken by Castile a few years previously, and the new king Alfonso was in a generally bellicose mood. By early 1255, however, Alfonso was deeply involved in his own crusade to invade North Africa.[60] In 1256, Jaume and Alfonso finally signed the treaty of Soria, settling most of their outstanding differences. Despite this or perhaps because of it, with Jaume able to turn his whole attention to Valencia, al-Azraq had the interview with Alfonso as reported in Jaume's memoirs. The Muslim became a client of Castile, persuaded Alfonso to pressure Jaume into a year's truce (Easter 1257 to Easter 1258), and confidently looked forward to a year's renewal of that truce. The Muslim's attitude, and what the king's memoirs reveal of the rebel's military position in 1258, suggests that al-Azraq had been ruling his mountain principality with impunity from at least 1252 into 1256. These dates roughly correspond with Alfonso's coming to the throne in mid-1252, making his truce with Granada, and carrying through a program of military pressure against Jaume's borders.

The meeting between al-Azraq and Alfonso was part of a larger pattern of semi-alliance between the two men. Al-Azraq requested Alfonso's intercession, for this truce of Easter 1258, around February or earlier. Alfonso received the Moor's banner on this occasion and sent his own. Jaume recorded that Alfonso had sent his banner to al-Azraq even before this time, "in such wise that he held him already under his protection" and undertook to safeguard him—"as al-Azraq recounted to his men and to others," who then told Jaume. Jaume's version of these events has the war dragging on "for three years or four" (actually much more, the war's full trajectory being included), when al-Azraq "negotiated first with Don Manuel, brother of the king of Castile," who governed Murcia to al-Azraq's south, "and then with the king of Castile."[61] The truce therefore marked the high point of an

Also in C. Bourel de la Roncière, *et al.*, eds., *Les Registres d'Alexandre IV*, 3 vols. (Paris, 1902–53), 1: doc. 653.

[59] Arch. Sec. Vat., Reg. Vat. 25, fol. 19v, no. 153 (12 February 1257) to the bishop of Valencia: "tua petitio nobis exhibita continebat [quod] ecclesia valentina magnis sit debitis obligata." Also in Bourel, *Registres*, 2: doc. 1,749.

[60] I have collected the documentation on Alfonso's relations with Valencia, after its conquest, in "Warrior Neighbors: Alfonso el Sabio and the Kingdom of Valencia: An Archival Case Study in His International Relations," *Alfonso X el Sabio: Vida, obra, época* (Madrid, forthcoming), with forty-one unpublished documents. Between 1253 and 1256, King Jaume had been anticipating "a war we expect to have" against the king of Castile (Cabanes, *Documentos*, no. 659 [30 August 1254]). The mutual compensations in 1256 for border damages seem to have been considerable and required international commissions to assess.

[61] Jaume I, *Llibre dels feyts*, chap. 371: "bé per tres anys o per quatre, que Alaçrac parlava pleit ab don Manuel frare del rei de Castella primer, e puis ab lo rei de Castella"; chap. 372: "en manera que

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informal relationship of some years, an alliance emboldening al-Azraq to threaten the king's lieutenant of Valencia.

Although we must read the king's self-serving memoirs with great caution here, the beginning of the stabilization or stalemate between Jaume and al-Azraq had been the recapture of Peñacadel by the Christians. After that victory, the Muslim forces held the mountains centered on al-Azraq's strongholds, while the king seems to have consolidated the rest of the kingdom by the use of settlements. In short, there were two Valencias, the king's and al-Azraq's. The Moor was self-sufficient in food and kept as well a powerful, salaried army. The situation might have persisted, a smaller version of the independent Granada on Castile's flank. The king's interpretation, that after this battle the Muslims gradually lost heart while Christian morale rose, seems naive.

A review of the early relationship between Jaume and Alfonso makes it appear very likely that al-Azraq's alliance with Castile backfired. Jaume might have suffered a mountain enclave as client, but he had always fiercely resisted Alfonso's intrusions (even as prince-heir) in that region. Al-Azraq's pact or understanding with Alfonso triggered Jaume's final campaign. It is significant that Jaume spends much time in his memoir on this episode of Alfonso, in an otherwise cursory account of the war, and that he built his campaign by raising false hopes of further truce and by suborning al-Azraq's counselor. Jaume's actions center on the Castilian intervention, and his account rings with bitterness toward Alfonso. Jaume even closes his history of the war with another story about King Alfonso. The Castilian on one occasion had sent for al-Azraq to meet him at Alicante in Murcia; a joke made there about al-Azraq capturing castles from Jaume, like a hunter taking game, reached Jaume and evidently rankled. Jaume's last act after al-Azraq's expulsion was to send a letter to Alfonso, taunting him with al-Azraq's loss of all he had to King Jaume as the better hunter.[62]

From September, 1257, the historiography of the al-Azraq crusade takes on an entirely new nature. The final campaign and destruction of this Mudejar state from late 1257 into 1258 coincides with the keeping of documentary registers by Jaume's chancery. Instead of a random parchment or some papal bulls, there are suddenly hundreds of charters kept on paper as legal alternates of the parchment copies sent. The relevant documentation is indirect: datelines of a siege, a number of allusions to "the traitor al-Azraq," time references such as "until al-Azraq leaves the land" or "the land the rebel al-Azraq now holds," feudal summonses to the post-Easter campaign of 1258, arrangements for the settlement of some of al-Azraq's land, notes about the siege of Alcalá, even a remark about the peace and treaty finally made with al-Azraq, and a comment on the transfer of the remnant of al-Azraq's holdings in a negotiated arrangement to his nephew Abū Ja'far.

els tenia ja en sa comanda, e, si pogués, que els defendria, segons que Alaçrac retrasc als hòmens seus, e a altres que ens ho dixeren."

[62] Jaume I, *Llibre dels feyts*, chaps. 371–77. Antonio Ballesteros Beretta, who knew Alfonso's itinerary and documents better than any other scholar, was inclined to place the original interview between Alfonso and al-Azraq, with its "hunting" jest, in July of 1254 (*Alfonso X el Sabio* [Madrid, 1963], 97, compare 177, 789).

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Equally important is the contextual documentation interspersed in these same registers, much more neglected. I have carefully combed all the registered charters from 1257 through 1263, a systematic search and reflection on those 500 documents for the Valencian kingdom. The pattern of loans, of garrisons, of obviously important Muslim estates granted, of Muslim authorities recently fled, of land surveying and distribution, of pensions, of gifts paid for but referring back to wartime largess, of wall repair, of settlement, and the like—all provide threads for weaving a larger tapestry than the more direct *obiter dicta* might allow. This research becomes a form of documentary archaeology—rescuing each minute bit of data, relating it to the context of the other bits and to the story as we know it, until a fuller account emerges. But that patient task requires a separate study. Suffice it to say here that King Jaume openly anticipated al-Azraq's defeat in early fall of 1257, and he labored to garrison his strongholds and finance his campaign during late 1257. The war is especially active in the documents from the summons of February, 1258, through the siege of Alcalá in late April into June, to the discussion of the final treaty of July.[63]

THE ATTEMPTS BY AL-AZRAQ TO RECOVER VALENCIA thus fell into three phases: a fairly general war that caused panic among the Christians and impelled them to seek the status and support of a crusade, then a stalemate during which al-Azraq ruled his mountain domain with impunity while Jaume mopped up the pockets of resistance elsewhere and consolidated territorial gains by settlements, and finally al-Azraq's alliance with Castile, which precipitated Jaume's bloody and relentless campaigns of 1258. The crusade status may have continued after the early 1250s, with the annual crusade-twentieth; there was no closure date for such a crusade except termination of the particular war. However, no papal documentation survives on this point; the clergy complaining of their poverty were probably reluctant to continue extraordinary aids indefinitely; the widespread program of importing Muslims suggests exhaustion and normalization of the general situation; and King Jaume's final convulsive campaigns appear to have been triggered suddenly, financed locally, and carried through with cold fury. Nevertheless, taken as a whole, and in its most extended and significant phases, al-Azraq's war and mini-state had been a preoccupation of wider Christendom, a genuine threat to its frontiers, and a crusade deserving of more attention in the series of thirteenth-century Spanish crusades.

[63] For a fuller discussion of the registers, their origins, nature, and problems, see my *Society and Documentation in Crusader Valencia*, especially chap. 9. For the methodology of "documentary archaeology," see my "Los Mudéjares de Valencia: Temas y metodología," *I Simposio internacional de mudejarismo* (Madrid, 1981), 453–97.

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Research Note

Slave Hiring in Texas

RANDOLPH B. CAMPBELL

HISTORIANS OF AMERICAN NEGRO SLAVERY have long been aware that slave hiring was a common practice across the antebellum South. Frederic Bancroft devoted a chapter to the subject in his *Slave-Trading in the Old South*, and virtually every general study has given it at least a few pages.[1] A knowledge of the hiring system has been especially useful to students of slavery in urban and industrial settings, as, for example, in Robert Starobin's work on industrial slavery and Richard Wade's and Claudia Goldin's respective works on slavery in the cities.[2] Robert Evans has provided the only substantial compilation of data on hiring rates, drawn primarily from the records of southern industrial enterprises. And Clement Eaton's pioneering article on slave hiring concluded that "hiring was the main method of introducing the slave into southern industry and city life—an important step toward freedom."[3] Slave hiring was not limited, however, to industrial and

[1] Frederic Bancroft, *Slave-Trading in the Old South* (Baltimore, Md., 1931), 145–64, 404–05. For typical references in well-known general studies, see Ulrich B. Phillips, *American Negro Slavery: A Survey of the Supply, Employment and Control of Negro Labor as Determined by the Plantation Regime* (New York, 1918), 204, 406–10; Kenneth M. Stampp, *The Peculiar Institution: Slavery in the Ante-Bellum South* (New York, 1956), 67–72, 84, 185, 204, 318, 414; Robert William Fogel and Stanley M. Engerman, *Time on the Cross: The Economics of American Negro Slavery*, 2 vols. (Boston, 1974), 1: 55–57, 103; and Eugene G. Genovese, *Roll, Jordan, Roll: The World the Slaves Made* (New York, 1974), 9, 390–92. An excellent recent synthesis and historiographical survey mentions hiring only in two places. See John B. Boles, *Black Southerners, 1619–1869* (Lexington, Ky., 1983), 122–23, 128–29.

Most of these studies also discuss briefly the practice of allowing slaves to hire their own time. This practice, whereby slaves paid their masters a set rate and then looked for work on their own at higher rates, must not be confused with the slave hiring system. Slave hiring was perfectly acceptable to whites, but slaves who hired their own time were thought to be exercising too much initiative and freedom. The Texas Legislature in 1846 made it illegal for a master to allow any slave to hire his own time for more than one day a week. H. P. N. Gammel, compiler, *The Laws of Texas, 1822–1897*, 10 vols. (Austin, 1898–1902), 2: 1501–02. This law was frequently violated, however. For typical editorial complaints about this problem, see (Austin) *Texas State Gazette*, 19 July 1851, 13 September 1851; and (Clarksville) *Northern Standard*, 20 December 1856.

[2] Robert S. Starobin, *Industrial Slavery in the Old South* (New York, 1970), 128–37; Richard C. Wade, *Slavery in the Cities: The South, 1820–1860* (New York, 1964), 38–47; Claudia Dale Goldin, *Urban Slavery in the American South, 1820–1860: A Quantitative View* (Chicago, 1976), 35–42, 69–75. The treatment of slaves hired for industrial labor is studied in Charles B. Dew, "Disciplining Slave Ironworkers in the Antebellum South: Coercion, Conciliation, and Accommodation," *AHR*, 79 (April 1974): 393–418.

[3] Robert Evans, Jr., "The Economics of American Negro Slavery, 1830–1860," in *Aspects of Labor Economics, A Conference of the Universities' National Bureau Committee for Economic Research* (Princeton, N.J., 1962), 182–243. Fogel and Engerman, *Time on the Cross*, 2: 25, mention a collection of slave hiring data from probate records, but Goldin, who used this information, still referred to Evans's data as the "only substantial compilation of hire rates." See Goldin, *Urban Slavery*, 69. Clement Eaton, "Slave Hiring in the Upper South: A Step Toward Freedom," *Mississippi Valley Historical Review*, 46 (March 1960): 663–78. Richard B. Morris, "The Measure of Bondage in the Slave States," *Mississippi Valley Historical Review*, 41 (September 1954): 230–36, deals with slave hiring in much the same way as Eaton.

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urban areas, which, after all, were a very small part of the antebellum South. The practice was also common on farms and in small towns where most southerners lived, and that aspect of the system has received little detailed study. One question in particular remains virtually unanswered: who hired slaves in rural areas and small towns? The answer is of considerable importance to understanding the role of slavery and the support it had in antebellum southern society.[4]

Judging from the records of Texas used in this study, many of the slaves who were hired out in rural, agricultural areas of the South came from the estates of deceased persons. Administrators and guardians who were seeking to pay debts or provide financial support for widows and minor children frequently hired out bondsmen belonging to estates under their control. Estate managers wishing to hire out slaves could almost always find a hirer. Examination of hundreds of estate records revealed only a handful of slaves who could not be hired. For example, an administrator in Washington County, Texas, who hired out twenty-six slaves in 1858 reported that John, his wife, and their four children were "not hired no one wanting them."[5] Other instances of failure to hire occurred in exceptional circumstances. Louisa R. Bennett, administrator of the Elisha D. Little estate in Washington County, reported in 1857 that she had sold a teen-age girl named Kate who had become "so vicious and unmanageable that when hired out no person to whom she was hired would keep her."[6]

Local probate courts required estate managers who engaged in slave hiring to file annual reports, which generally provided information on the slaves involved and named the individuals to whom the bondsmen were hired.[7] These reports, preserved in county courthouses across the South, constitute an unmatched basic source for a systematic investigation of the slave-hiring system. This research note, using Texas as a test case, first demonstrates the use of probate record data to determine who hired slaves in rural, agricultural areas. Then, with information drawn from census returns and tax rolls, it analyzes the demographic and economic characteristics of those hirers.

Historians have long known about probate records as a source for research into

[4] Boles, *Black Southerners*, 229–31, does not list any studies of hiring in rural, agricultural areas. Andrew W. Foshee, "Slave Hiring in Rural Louisiana," *Louisiana History*, 26 (Winter 1985): 63–73, deals with the subject only tangentially as it relates to theories about food self-sufficiency and does not offer any new data on slave hiring or information on who hired slaves. Sara S. Hughes, "Slaves for Hire: The Allocation of Black Labor in Elizabeth City County, Virginia, 1782–1810," *William and Mary Quarterly*, 35 (April 1978): 260–86, is the only study within the past ten years that deals with who hired slaves and, in spite of its title, its systematic investigation of that particular question is limited to a period of three years (1784–1786) in one Virginia county (see pp. 268–69).

[5] Estate of William Routt, Washington County Probate Records, Final Record, Book G. One other example of the inability to hire out slaves was found in Cass County. Estate of James Wasson, Cass County Probate Records, Final Record Probate Court, Book 3.

[6] Estate of Elisha D. Little, Washington County Probate Records, Final Record, Book G.

[7] It is impossible to determine the exact proportions of slaves hired out by estate managers and by living slaveholders, but the evidence is strong that slaveholders preferred to manage their slaves themselves rather than make them available for hire. Fogel and Engerman, *Time on the Cross*, 2: 24–25, indicate that probate records were their primary source for hiring data. Two state-level studies, Orville W. Taylor, *Negro Slavery in Arkansas* (Durham, N.C., 1958), 82, and Ralph B. Flanders, *Plantation Slavery in Georgia* (Chapel Hill, N.C., 1933), 194, said that estates undergoing administration or settlement were the primary sources of hired slaves. For evidence on slave hiring, Abigail Curlee, "A Study of Texas Slave Plantations, 1822–1865" (Ph.D. dissertation, University of Texas, 1932), 58–76, cites estate records far more often than the plantation journals used extensively elsewhere in her study.

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social and economic questions and have made extensive use of these materials in colonial history.[8] The term "probate records" actually covers a variety of county archival materials. Some counties maintain files of the original case papers created as an estate went through probate. These report in detail all actions taken by estate managers and the court, but they are often in deteriorating condition and difficult to read. Fortunately, most counties maintain bound volumes containing recorded copies of the probate papers. These books are usually called probate records or estate records and are generally well preserved and reasonably easy to read.[9] Many counties also have probate dockets and probate minutes in bound volumes, but these usually contain no details beyond brief statements of actions taken by the court.

The hiring-out of slaves by estate managers occurred everywhere that slavery existed in antebellum Texas. In areas with relatively few slaves, however, there were not enough cases to justify a search of the probate records for slave-hiring data. Therefore, this investigation is limited to the portion of eastern and south-central Texas where slaves generally constituted at least one-quarter of the population by 1860 and slave hiring was common enough to generate a sizable number of reports by estate managers in the probate records of most counties.[10] More precisely, the area of investigation is marked by a line running southwestward from Lamar County on the Red River to Calhoun County on the Gulf coast (see map). In 1860, with the exception of Falls and McLennan counties, every Texas county having slaves as 25 percent or more of its total population was to the east of that line. Twenty counties were chosen according to geographical location to represent this region of extensive slave populations. These sample counties contained 74,529 slaves in 1860, approximately 41 percent of all the slaves in Texas.[11]

[8] Alice Hanson Jones, *Wealth of a Nation to Be: The American Colonies on the Eve of Revolution* (New York, 1980), for example, depends on a sample of probate inventories drawn from counties existing in all the thirteen colonies in 1774. Jackson Turner Main, *Society and Economy in Colonial Connecticut* (Princeton, N.J., 1985), also draws heavily on probate materials.

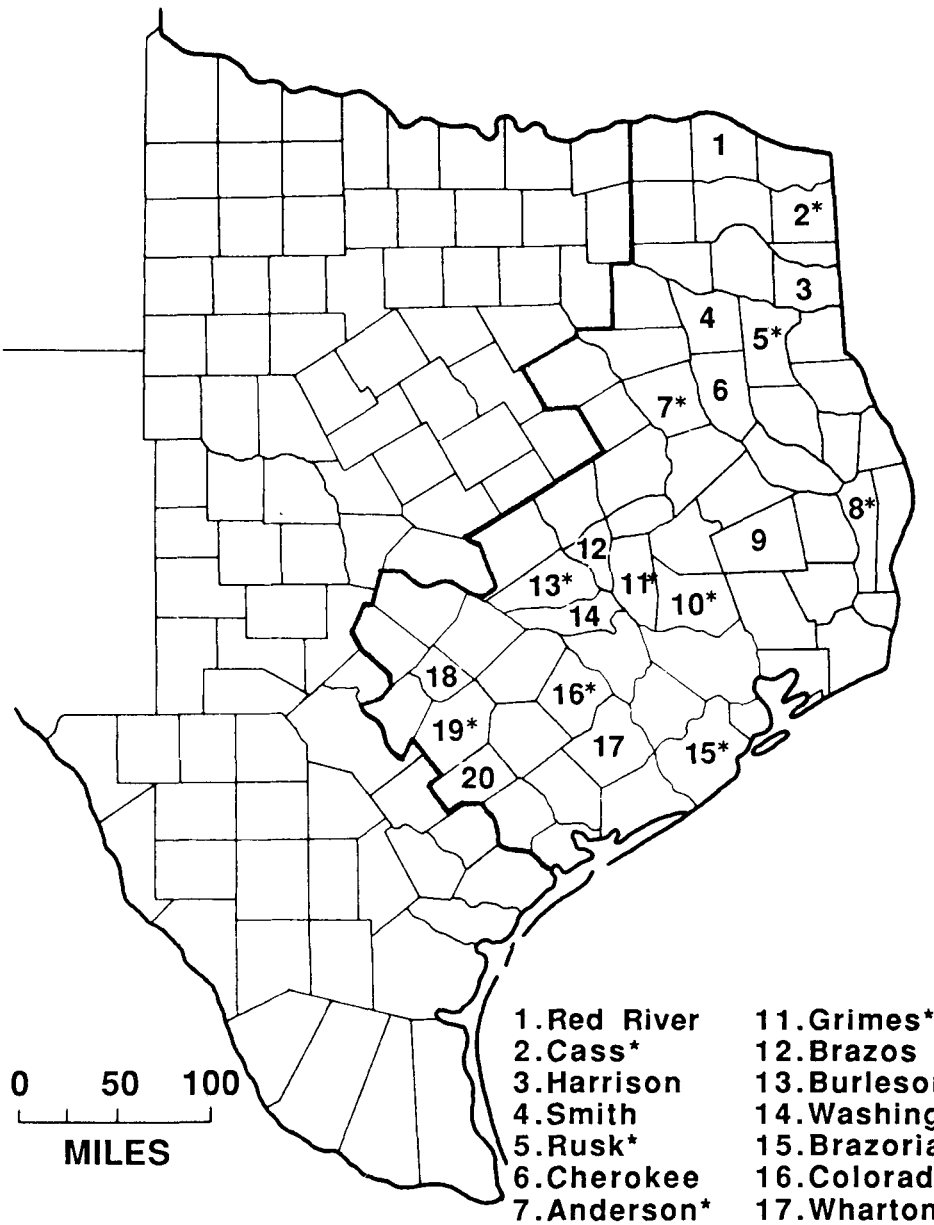
[9] In this discussion, references to "probate records" are to the bound volume copies of the original probate papers. A great many of these record books have been microfilmed by the Mormon church as part of its genealogical work. The state of Texas has purchased copies of this microfilm and makes it available to researchers in the state's various Regional Historical Resource Depositories. A total of twenty-five depositories, primarily university or metropolitan libraries, serve all but a few of Texas's 254 counties and through a loan system make it possible to use county records without the time and expense of extensive travel.

[10] State law governing the handling of estates of deceased persons and thus the creation of probate records is found in Williamson S. Oldham and George W. White, *A Digest of the General Statute Laws of the State of Texas* (Austin, Tex., 1859), 165–201. Guardianship is discussed in pp. 228–36. For a brief period from 1846 to 1848, Texas had special probate courts, but after August, 1848, the county court had probate jurisdiction with the county judge serving *ex officio* as probate judge. H. P. N. Gammel, comp., *The Laws of Texas, 1822–1897*, 10 vols. (Austin, Tex., 1898–1902), 2: 1614–24; 3: 113, 118, 235–97.

[11] Antebellum Texas, although overwhelmingly rural and agricultural, had the beginnings of towns and industries, which used urban and industrial slaves, some of whom were hired. See Kenneth W. Wheeler, *To Wear a City's Crown: The Beginnings of Urban Growth in Texas, 1836–1865* (Cambridge, Mass., 1968), 106–10, 143–48; Earl Wesley Fornell, *The Galveston Era: The Texas Crescent on the Eve of Secession* (Austin, Tex., 1961), 118–22, 230; and Paul Dean Lack, "Urban Slavery in the Southwest" (Ph.D. dissertation, Texas Tech University, 1973), 30–33, 58–59, 73–74. None of the major towns in antebellum Texas, however, were located in the counties on which this research note is based. Summary statistics on slaveholding in Texas as reported in the census of 1860 are found in U.S. Bureau of the Census, Eighth Census, *Agriculture in the United States in 1860; Compiled from the Original Returns of the Eighth Census* (Washington, D.C., 1864), 240–42. The county outline map of Texas in 1860 is the work of Terry G. Jordan, Department of Geography, University of Texas at Austin.

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TEXAS 1860



1. Red River
2. Cass*
3. Harrison
4. Smith
5. Rusk*
6. Cherokee
7. Anderson*
8. Jasper*
9. Polk
10. Mongomery*
11. Grimes*
12. Brazos
13. Burleson*
14. Washington
15. Brazoria*
16. Colorado*
17. Wharton
18. Caldwell
19. Gonzales*
20. DeWitt

*Counties examined for biographical data on slave hirers

TGJ

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A sample of 623 individuals who hired slaves during the period from 1848 to 1862 was drawn from the probate records of these twenty sample counties. County tax rolls reveal whether or not those 623 hirers were property holders in the county in which the hiring took place and, if so, the nature and extent of their property.[12] These records were used to determine whether the hirers were slaveholders or nonslaveholders. Three-quarters (463) of the hirers could be positively identified on the tax rolls of the twenty counties for the years during which the slaves were hired.[13] Of these, 275 (59 percent) were slaveholders, while 188 (41 percent) did not pay taxes on slaves during the year they hired bondsmen. Analysis was limited to hiring during the years 1848 to 1862 because, by 1848, slaves were numerous enough in most of these counties to create reasonably large numbers of hire cases and because, by the end of 1862, the currency system of Texas was being upset by the Civil War.

Slaveholders, who represented only about 33 percent of all heads of household in the part of Texas represented by the twenty sample counties, obviously were overrepresented (at 59 percent of the total) among the hirers. The percentage of nonslaveholders was nevertheless large enough to be of special significance. Forty-one of every 100 hirers identified from the probate records of these twenty counties did not own slaves. Unfortunately, there is no way to determine exactly how many Texans hired slaves from estate managers or slaveholders in any given year or period. But, if this proportion of nonslaveholder hirers held true for the state's extensive hiring system, and I have no reason to believe that it did not, far more Texans had direct involvement with slavery than would be indicated in records of ownership such as the census or tax rolls. Indeed, it seems likely that, at one time or another during the antebellum years, thousands of Lone Star State households benefited from slave labor without actually owning a single bondsman. This circumstance would not alter the concentration of wealth in Texas or change the relative positions of planters and plain folk, but it would be an additional explanation of the strong support for slavery in a state where only a little more than one-quarter of all families owned slaves in 1860.[14]

[12] Far more than 623 hiring agreements were reported by estate managers, but the sample was drawn only from those for which at least the gender and hire rate for individual slaves and the names of individuals making the hires could be determined. The gender and hire rate data were necessary for other aspects of a broader study of the hiring system in Texas. Tax records for mid-nineteenth-century Texas are available on microfilm on a county-by-county basis. Records of the Comptroller of Public Accounts, Ad Valorem Tax Division, County Real and Personal Property Tax Rolls, Archives Division, Texas State Library, Austin.

[13] A variety of factors explain the failure to locate 25 percent of the hirers who were named in the probate records. Some did not live in the county in which the hiring took place, and it was simply too time-consuming to search the tax rolls of all surrounding counties. (A few hirers were found in neighboring counties, thanks to indexes to the 1850 and 1860 censuses that gave clear indications of where to search.) Others who were designated as "not located" were the wives or sons of household heads. It could not be determined if they were acting on their own or for the head of household who actually appeared in the tax rolls.

[14] The possibility that the hiring system broadened support for the Peculiar Institution among individuals who did not appear as slaveholders in census or tax records is emphasized in James Oakes, *The Ruling Race: A History of American Slaveholders* (New York, 1982), 40. The minority position of slaveholders in Texas is documented in Randolph B. Campbell and Richard G. Lowe, *Wealth and Power in Antebellum Texas* (College Station, Tex., 1977), 43–45. Hughes, "Slaves for Hire," 268, found that only 21 percent of the hirers in Elizabeth City County, Virginia, in 1784–1786 were nonslaveholders.

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In order to determine the social and economic characteristics of those hiring slaves, it was necessary to make an examination of the U.S. Censuses of 1850 and 1860 for biographical information on slave hirers and to use the tax rolls to determine the changing economic fortunes of those same individuals over the years. Because of the time required to locate particular individuals in these manuscript records, this part of the study was limited to slave hirers in ten of the twenty sample counties, which were again chosen to give reasonable geographical coverage to the area of extensive slave population (see map). These ten counties contained 319 (51 percent) of the 623 hirings reported in the original sample. Eighty-one hirers could not be located on the tax rolls of the county in which they first hired slaves. Once this group was removed from consideration, the remaining 238 individual hirers, 140 slaveholders (59 percent) and 98 nonslaveholders (41 percent), became the subject of special searches in the census returns and tax rolls. First, I made an attempt to locate them in the censuses of 1850 and 1860 with the intention of determining the age, location of birth, occupation, and property-holding status of each. Second, each of the 238 hirers was sought five years after the date of the original hire on the tax rolls of the county in which the hirer was first located.[15] Those who hired less than five years before 1862 were sought on the 1862 tax rolls. For example, an individual who hired slaves in Cass County in 1850 and was located on the tax rolls that year was sought on the 1855 Cass County tax rolls; one who hired in 1858 was sought in 1862. Information on land ownership, slave ownership, and total taxable property was recorded for both years.

Census data indicate that slave hirers in antebellum Texas living in an area of extensive slaveholding differed from the average heads of household in the same region in several ways. They were much like the typical household heads in terms of age but differed somewhat in birthplace and occupation and were notably wealthier. The average age of slave hirers at the time of hiring was 39.3 years; across the area of extensive slaveholding, the average age of household heads was 38.3 in 1850 and 39.2 in 1860. The vast majority (89.9 percent) of slave hirers were natives of the South, compared to approximately 78 percent of all heads of household in the area of extensive slaveholding. Nearly two-thirds (65.7 percent) of the slave hirers were farmers, whereas, among household heads in this area of Texas, approximately 70 percent listed agricultural occupations in the censuses of 1850 and 1860. Merchants and professionals, however, who headed only 9.6 percent of households in this region at both census years constituted 15.4 percent of the hirers in the period 1848–1852 and 17.5 percent in 1858–1862. Individuals who hired slaves between 1848 and 1852 reported mean real property holdings worth $2,688 in 1850, an amount that was 69 percent greater than the mean of

[15] Seventh Census of the United States, 1850, Schedule 1—Free Inhabitants; Eighth Census of the United States, 1860, Schedule 1—Free Inhabitants. The ten counties for which these microfilmed census reports were examined for slave hirers are indicated on the map. Real and Personal Property Tax Rolls for the same ten counties were also examined. The search located 207 (87 percent) of the 238 hirers in question in the census and 180 (76 percent) on the tax rolls five years later. The number not located later on the tax rolls was increased by the inclusion of those who hired for the first time in 1862 and could not be sought later.

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TABLE
Mean Ages and Property Holdings
for Heads of Household and Slave Hirers

	Age	
Head of Household from 1850 Census (N = 3,909)	Heads of Household from 1860 Census (N = 3,299)	Slave Hirers in 1848–1862 (N = 204)
Mean = 38.3	Mean = 39.2	Mean = 39.3
SD = 11.0	SD = 11.6	SD = 9.2
	Real Property in 1850	
Heads of Household from 1850 Census (N = 3,909)		Slave Hirers in 1848–1852 (N = 39)
Mean = $1,595		Mean = $2,688
SD = $5,165		SD = $4,175
	Real and Personal Property in 1860	
	Heads of Household from 1860 Census (N = 3,299)	Slave Hirers in 1858–1862 (N = 114)
Real Property	Mean = $3,164	Mean = $6,168
	SD = $11,438	SD = $14,393
Personal Property	Mean = $4,465	Mean = $6,987
	SD = $11,380	SD = $11,971

The table indicates the range of ages and property holdings summarized by the means presented in the text. The standard deviations (SD) indicate that there was considerable variation from the means for age, real property, and personal property. It should be noted, however, that the standard deviations for the slave hirers were uniformly smaller relative to the mean than were standard deviations for the means based on the census samples. This difference suggests that the means for hirers were less affected by extreme cases than were those for the general census samples and were reasonably reliable indicators of age and property-holding characteristics.

$1,595 held by typical household heads in that area of Texas. In the 1860 census, those who hired slaves between 1858 and 1862 reported a mean wealth holding (real and personal property combined) of $13,155. This was 72 percent more than the $7,629 that was the mean in the same area of Texas. Thus, while there was much individual variation, slave hirers were not generally young men trying to get rich; instead, most were already wealthier than average (see table).[16]

[16] These comparisons concerning birthplaces, occupations, ages, and property holding are based on information in samples taken from the censuses of 1850 and 1860 by myself and Richard G. Lowe for

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Information from the tax rolls concerning the economic status of persons five years after they first hired slaves suggests that slave hiring did not necessarily bring improved fortunes. In the case of landholding, for example, 33 percent of slave hirers had more acreage five years after first hiring slaves, but 42 percent held the same amount of acreage, and 25 percent held less. Many (40 percent) of the slaveholders held more bondsmen, but 19 percent held fewer. Thirteen (also 13 percent) of the nonslaveholders had joined the slaveholding class, but the great majority (87 percent) still owned no bondsmen. Among the entire sample of hirers, more than half (57 percent) had increased the total tax value of their property, but 43 percent were judged by the tax assessor to be worth less than when they had first hired slaves. Thus slave hiring was hardly the road to riches. Nonslaveholders who hired slaves appear to have been a little more likely than slaveholder hirers to improve their economic status over the years. Among those located in the tax rolls five years after first hiring, 69 percent of the nonslaveholders showed an increase in taxable property value. The comparable statistic for slaveholders was 51 percent. Of course, the slaveholders generally had more taxable property in the first place.

IN CONCLUSION, A SYSTEMATIC INVESTIGATION based on data drawn from the reports of administrators and guardians in probate records in twenty Texas counties strongly suggests that, although hiring was not necessarily a step toward economic gain, many men and women across rural, agricultural Texas stood ready to hire slave labor whenever it was available. Natives of the South and individuals with commercial and professional occupations were overrepresented among hirers. Hiring was not limited to a particular economic class, but those who hired bondsmen were typically wealthier than the average head of household in the area of Texas investigated. Finally, and most significant, hiring gave many nonslaveholders a means of participating in the slave system, thereby increasing involvement and potential popular support beyond the limits indicated by statistics on slave ownership.[17] Slave hiring, as revealed in this investigation of who hired bondsmen, both exemplified and contributed to the vitality of the Peculiar Institution in Texas on the eve of secession.

our study, *Wealth and Power in Antebellum Texas* (see pp. 17–31 for a discussion of the sampling procedure). These samples consisted of 5,000 heads of household from the 1850 census and 5,000 from the 1860 census for antebellum Texas (defined as the eastern two-fifths of the state because only that area was truly settled before the Civil War). When these samples were reduced to include only data drawn on household heads in counties in the area of extensive slaveholding investigated in this study, the results were subsets of 3,909 cases in 1850 and 3,299 in 1860.

[17] Barbara Jeanne Fields, *Slavery and Freedom on the Middle Ground: Maryland during the Nineteenth Century* (New Haven, Conn., 1985), 84, suggests that hiring may have indicated a weakening of slavery economically in Maryland, in that hiring was a way for owners to shift the costs of supporting slaves when their labor was not necessary. Only one example supports this argument, however, and there is no indication that hiring had such a purpose in Texas.

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Reviews of Books

GENERAL

GEOFFREY PARKER, editor. *The World: An Illustrated History*. New York: Harper and Row. 1986. Pp. 480. $25.00.

One of the more successful ploys of the mass media in our time is the educational package: a television show on warfare or the universe or modern art, also available in cassette form, with an accompanying illustrated book equally appropriate for coffee tables or classrooms. Such packages can be stimulating or routine, pompous or bland. In any case they are big business, and they compete briskly with the traditional high school or college course of live lectures by living pedagogues.

The present book is part of one such package, produced in Great Britain and featuring a documentary film series in twenty-six parts, "The World: A Television History." Not by coincidence, the book has twenty-six chapters, each chapter corresponding to a television program in the series, and each consisting of material "provided by the same contributing historians who wrote the original scripts, according to the general plan laid down by the late Geoffrey Barraclough" (p. 5).

These "contributing historians" are identified by name in the front matter of the book—nearly all are British scholars—but the editor, Geoffrey Parker, provides no hint of who wrote what. Presumably, since there happen to be twenty-six contributing historians, each contributor was responsible for one chapter. But it is difficult to reconstruct authorship. All twenty-six chapters read as if they had been written by the same person, in the same clear, competent, colorless, slightly disapproving prose. True to the subtitle, the book is also fitted out with a lavish array of illustrations, many in color, which fill half its pages.

Parker tells us in his introduction that the premise of the project was a firm resolve to avoid "telling the whole story in terms of the rise of the West" (p. 5), presumably not intended as an attack on *The Rise of the West* by William McNeill, one of the twenty-six contributing historians. To this resolve Parker and his collaborators remain admirably faithful. Of the twenty-six chapters, ten concern the West, eight the non-Western world, and eight treat both. The volume is also not tilted toward the events of modern history. Prehistory and world history to 1500 occupy sixteen chapters, the modern world only ten. With a few exceptions, such as the Byzantine empire after the fall of Rome and Latin America since independence, every part of the world and every period of history receive their fair share of attention.

There is also no excess of pro-Western sentiment. The ravaging of Africa and pre-Columbian America by the West is reported without any effort to spare Western feelings, and the point is well taken that even as late as the eighteenth century most of the world's people, wealth, and cultural achievements were to be found in Asia, not in Europe.

Yet in another sense *The World* is a deeply conservative, narrowly political, and intransigently male digest of world history. In the premodern chapters most of the interest centers on emperors and warlords, their battles, and the rise and fall of their realms. Warfare and politics also bestride the last part of the book. Social and economic history receives little consideration, the history of women none. In the chapter on early modern Europe, its progress to world domination is explained solely as the result of "the emergence of the modern state" (p. 279). Economic matters are dismissed in two paragraphs, and capitalism is scarcely mentioned. Indeed, in the last chapter, a telling reference is made to "the 'capitalist' West" (pp. 465–66). The quotation marks imply that the whole notion of capitalism is an invention of paranoid leftists. From the possibility of other points of view, or contending theories of world history, the reader is scrupulously protected.

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THEODORE M. PORTER. *The Rise of Statistical Thinking, 1820–1900*. Princeton: Princeton University Press. 1986. Pp. xii, 333. $35.00.

Theodore M. Porter's book is the latest contribution to a growing field of study that exists in the intersection of intellectual history, the history of social thought, and the history of mathematics. When and why did Western culture adopt the habit of describing social reality in quantitative terms? How did the propensity for number gathering change the way social reality was perceived? And, most intriguing, how did the early statistical enthusiasts reconcile apparent regularities in social data, which seemed to suggest that the frequencies of certain social phenomena were fixed, with the concept of free will?

Porter looks closely at the leading theoreticians of the early statistical movement, a movement generally characterized by uncritical zeal for numbers and rather simple mathematical formulations. His story moves swiftly from the 1820s to the 1840s, first focusing on Adolphe Quetelet, the Belgian who borrowed the astronomers' "error law" (the normal distribution curve) and fit it to human physical characteristics as well as moral behavior. Others after Quetelet moved beyond his naive assumption that the mean value, describing the "average man," expressed the ideal of humanity and that deviations from the mean were errors or flaws. But what many of these early proponents of statistics did share in the middle decades of the nineteenth century was a faith in the law of large numbers, which predicted that statistical regularities would surely emerge, given sufficiently large observations of social data. The surprising regularity in rates of suicide by hanging was but one of many examples in which statistics seemed to confer order on a chaotic world.

But how could predictable regularities in events thought to be wholly a matter of chance or whim be consistent with free will? Porter devotes two substantial chapters to the cautious thinkers who were troubled by the determinism implicit in statistical regularities. His extensive study in German and French sources is impressive, and he shows how the philosophical debate over statistics contributed to the decline of determinism in the late nineteenth century.

Porter also restores balance to what historians have often assumed to be a one-way path of ideas. Quetelet and other proponents of statistics took pride in elevating the study of social phenomena to a science by borrowing the language of mathematics and the methods of measurement. But in so doing they popularized new mathematical formulations, such as the law of large numbers, which then were transported back to the natural sciences. Key chapters of the book describe how physicists and biologists such as James Clerk Maxwell, Ludwig Boltzmann, and Francis Galton borrowed from the developing statistics of social science to explain aggregate behavior of phenomena that on an individual level appeared to be random and unpredictable, such as the behavior of molecules in kinetic gas theory.

In the introduction Porter promises that his book can be understood even by readers with no mathematical training, but this is not really true. He assumes a basic familiarity and comfort with such concepts as a normal distribution curve, standard deviation, variation, correlation, regression, and probability, and anumerate historians whose faces blanch at these terms should best study them before embarking on this book. Porter very helpfully supplies a list of nineteenth-century synonyms for contemporary terminology at the outset, which forestalls some potential confusion.

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PETER GAY. *The Bourgeois Experience: Victoria to Freud*. Volume 2, *The Tender Passion*. New York: Oxford University Press. 1986. Pp. viii, 490. $24.95.

This enjoyable volume continues Peter Gay's exploration of the bourgeois world, particularly its erotic core. It features the writings of Honoré de Balzac, Charles Dickens, Emile Zola, and Charles Kingsley along with a rich cast of minor witnesses including horticulturalists, epistolary as well as pious and poetic writers. Less gloomy than Denis de Rougemont, less severe than Philip Rieff, Gay weaves his narrative way through repressions, prohibitions, and displacements to demonstrate once again that in spite of it all the bourgeoisie was successful in love and experienced it intensely. In two volumes of literary analysis he has built a case that too close attention to silences and displacements has hitherto confused historians about a constant of bourgeois life: the persistent experience of orgasmic content. At the end of this literary sexual symphony one has experienced a text full of pleasure, titillation, amusement, shock, and instruction—all in happy combination.

Gay also continues shaping material according to Freudian categories and insights. This interesting and welcome heuristic, however, provokes a psychologically oriented viewing of what the author has in mind and what he has accomplished. There is little doubt that Gay has created a text mirroring the pleasure and play he hoped to find in the nineteenth century. The dense display of sexual activity leaves the reader ready for some respite from an eroticism without spaces and without other consequences than more pleasure. Such a display reminds

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us of the narcissistic drive for mastery and domination that remains absent from Gay's other work. Having searched for an echo in the Enlightenment and in Weimar culture, he finds it instead in such nineteenth-century characters as Mabel Todd and Kingsley. Yet, to restore this world of pleasure, Gay has had to break with Freud. Shattering his master's profound and ultimately mature pessimism, Gay challenges the accuracy of Freud's observations and faults his blindness to the "openness" and pleasures of bourgeois eroticism. In so doing, Gay opts for the world of the polymorphously perverse with his undifferentiated identifications, especially with Todd. Resolving all tensions in orgasm, he refuses the stern father and settles for an ecstatic bourgeoisie—satisfied as well as self-satisfied.

Gay's continued fusion with Todd ultimately distorts the writing of history, which demands distance and differentiation, to say nothing of chronology. Chronology is strikingly absent from this history as it is from the terrain of polymorphism. Moreover, all witnesses support the celebration of triumphant eros, whereas otherness disappears. Gay is utterly, if not willfully, blind to developments in the history of women, for example. In this volume no woman's writing receives careful consideration—not Elizabeth Browning, not the many lesbian writers of the Belle Epoque. Except for a few pages on Beatrice Webb, the text superficially invokes female authors, whereas it carefully examines the cast of male writers. The case of Todd marking identity, an erotic literature emerges based on a discourse by the male absolute without a female counterpart. Furthermore, Gay blindly closes his eyes to the evidence of difference so puzzling to Freud. Thus, he maintains that women's participation in the crusade against prostitution was widely acclaimed because the effort filled a psychic need to reclaim maternal innocence. As evidence he musters a few female crusaders who flit textually around the central one, William Gladstone. Yet how could a historian omit the story of Josephine Butler, known together with her collaborators as "the shrieking sisters"? Perhaps Gay's narrative positioning has not reached a psychic stage from which he can see, much less acknowledge, the general opprobrium surrounding Butler. The same is true for similar efforts, such as that of Lida Gustava Heymann and Anita Augsburg in Hamburg. Seeing paradoxes in all his texts and in the lives of his protagonists, Gay nonetheless refuses or is blind to difference. Thus, the book looks playful and sexy, but the arena for that play is a dangerous and regressive one.

Finally, Gay's nineteenth century devolves from an unproblematic understanding of reading and writing. He has no difficulty reading the quality of sexuality in written works. From writing back to pleasure, from the ego back to the id or the unconscious is even more trouble free a process than orgasm. If Gay has rejected the pessimism in Freud ("much will be gained if we succeed in transforming your hysterical misery into common unhappiness"), neither does he see any difficulties in exploring the language of pleasure or any language for that matter. We are back in the prelapsarian world of the Enlightenment, where the only troubles arise from superstitions and their high priests—in this case historians who, although they recognize that sex saturated the nineteenth-century vision of things, refuse to celebrate it or join in the ecstasy.

Despite its closeness, the nineteenth century remains an other, different and always in need of historical prudence. A time of epidemic venereal contagion that produced silence as well as outrage and a time of sexual engagement like all other times, the century and its sexual practices demand distance, conversely, in approaching so intimate a topic. Michel Foucault reminded us early on of the consequences of sexuality in its written form. We can hardly ignore the hard-won insight that writing about the body and describing the historicity of its pleasures is a delicate undertaking. In this regard Gay's account, for all its richness and moments of explanatory brilliance, remains prurient rather than critical, participatory rather than historical.

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ANNABELLE M. MELVILLE. *Louis William DuBourg: Bishop of Louisiana and the Floridas, Bishop of Montauban, and Archbishop of Besançon, 1766–1833*. Volume 1, *Schoolman, 1766–1818*; volume 2, *Bishop in Two Worlds: 1818–1833*. Chicago: Loyola University Press. 1986. Pp. xvi, 505; 507–1065. $34.95 the set.

In her prefatory remarks Annabelle M. Melville argues that, whereas no previous historian has produced a definitive study of the life of that notable Roman Catholic churchman Louis William DuBourg (1766–1833), neither should her two-volume work be considered as such. Melville's view does not come from a false sense of intellectual honesty. Rather, it is based on a genuine appreciation of the breadth of Bishop DuBourg's involvement in the Catholic affairs of both France and the United States and the resultant accumulation of a plethora of archival documentation on him that scholars would have to investigate. Throughout her detailed account, however, Melville exhibits a sweeping familiarity with those sources. Evidencing a thorough knowledge of the main historical movements of late eighteenth- and early nineteenth-century Europe and the Americas, she has written more than just a biography of an important ecclesi-

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astical figure. She has styled a perceptive narrative that, centering on Bishop DuBourg, sheds light on a number of aspects of the condition of the Catholic faith in France from the fall of the *ancien régime* through the early years of the July Monarchy and in America from the Federalist period into the 1820s.

The division of the history into two volumes was handled in an intelligent manner. As with the lives of any number of Catholic clerics who went on to become bishops, DuBourg's story evolved in such a way as to suggest a seemingly natural separation at the point at which he was elevated to the episcopacy. Melville, however, wisely chose to divide her effort into two volumes not at the date of DuBourg's consecration as bishop of Louisiana and the Floridas on September 24, 1815, but instead at the time when the new ordinary actually took possession of his episcopal see of St. Louis on January 5, 1818. Thus, Melville focuses more on the prelate's role as bishop of a large western frontier diocese than on the actual fact of his becoming a member of the Catholic hierarchy.

Volume 1 develops the bishop's early years as well as his family background. The author carefully traces the Frenchman's growth as a cleric, emphasizing his religious formation as a member of the Congregation of St. Sulpice (founded in 1642) and the development of his outstanding capabilities as an educator and molder of young minds. Melville deftly measures her subject's strengths against his weakness, as the future bishop emigrated from France to the Baltimore area and subsequently served as president of Georgetown College, founded St. Mary's College of Baltimore, and helped establish Mother Elizabeth Seton's school for girls at Emmitsburg.

Volume 2, although acknowledging that DuBourg was not only a deeply spiritual person but also a man of great intellect, centers on his character as a bishop who presided over dioceses in both the United States and France during very difficult times for the Catholic church. As bishop of Louisiana and the Floridas, DuBourg was called on to rebuild a religion that had all but disappeared during the years of the War of 1812. Melville clearly stresses the prelate's importance in that role. During the latter stages of his life DuBourg was called on to act as bishop of Montauban and archbishop of Besançon in his native France, as that nation experienced the trauma of the Bourbon restoration and the July revolution.

In an age when many historians question the validity of major research on episcopal figures, Melville has produced a work that goes a long way toward refuting that position. Establishing a high standard for current historians working on the biographies of other Roman Catholic ecclesiastics such as Antoine Blanc of New Orleans, Jean Marie Odin of Galveston, and John Timon of Buffalo, Melville has produced a first-rate scholarly study written in a very readable style. Anyone interested in that era should read her two volumes with zeal, and no Catholic scholar should ignore her work.

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NORMAN ROSE. *Chaim Weizmann: A Biography*. New York: Elisabeth Sifton of Viking. 1986. Pp. xiv, 520. $24.95.

This is an eminently well written biography that will be read with delight well beyond the academic community. Specialists need anticipate few revelations. Norman Rose's goal is to "restore Weizmann to his rightful place in the Zionist pantheon" (p. viii). Emphasis is on the period up to World War II, when Weizmann was at the peak of his powers. Less than one-quarter of the text is devoted to the crucial last decade of the Palestine Mandate.

This volume will inevitably be compared with the two-volume biography being written by Jehuda Reinharz, the first volume of which (reaching 1914) came out too late to be of benefit to Rose. In contrast to the wealth of archival material used by Reinharz, Rose's work rests overwhelmingly on the *Weizmann Letters* series (twenty-two published volumes), contemporaries' memoirs, and secondary works. Rose confirms Reinharz's finding that Weizmann's autobiography is not always reliable, ("as reliable as most") but regards it as "an authentic contribution to contemporary history" and indeed relies on it heavily (p. 456). The result is a highly personalized, single-perspective story.

Like Reinharz, Rose finds Weizmann's position on the so-called Uganda scheme ambivalent. Weizmann's initial support soon faded: "By opposing the East Africa plan he could raise his own status and, with the cause of Palestinian Zionism to fight for, the Democratic Faction would be revitalised" (pp. 71–73).

Weizmann's personal role in securing the Balfour Declaration has been the subject of much historical debate. As Rose sees it, Weizmann was cast, almost by divine providence, at the center of events in London, at a time when the Zionist movement was hopelessly divided. It would be only a matter of time and circumstance before the vacuum would be filled "by a strong-willed personality with a clear-cut policy. Weizmann fitted the bill" (p. 137). Sir Charles Webster's verdict, "Weizmann's feat was the greatest diplomatic coup of the First World War," is quoted without reservation (p. 186).

Weizmann's influential drawing-room diplomacy is analyzed elegantly by Rose. Brought to London by

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the government to work on acetone, Weizmann was soon moving "in the stratosphere of high politics . . . select dinner parties and fine talk, idle chatter and good manners, impeccable breeding whether the background was land or money . . . He engaged and exploited their society but was not seduced by it. For them he was a tolerated outsider, a Russian-born Jew, a naturalized British subject who could raise them above their day-to-day cares with his vision of the restoration of the Jews in Palestine" (p. 149). Yet Rose himself concludes that all this "impressive socialising" did not in fact amount to much in concrete political terms. He concludes that "the British would discover Zionism in their own time, and for their own reasons" (p. 151).

Chapter 12, ("The Chief"), a flowery pen-portrait, is definitely the high point of the volume. Weizmann was possessed of an optimism that "tended to race ahead of reality" (p. 95). Yet, like other great statesmen, Churchill for instance, he suffered severe depressions. (Weizmann's were called "fogs," Churchill's "the Black Dog"). Weizmann's depressions came more frequently with old age, when poor health and political betrayal were his lot.

Rose refers to Weizmann's "propensity to form *liaisons* with other women" at times ridiculously too young for him (p. 234). Rose suggests that "reclaiming lost souls for Judaism was, apart from the intellectual challenge, a gambit for Weizmann in approaching women who aroused him" (p. 116). Weizmann's flirtations wounded and humiliated his wife and led to marital crisis, even if Vera remained confident in her position as wife. They separated for nearly a year, from 1925 to 1926. But Vera's attachment was more than simply emotional: "Vera *was* enamoured, even intoxicated with the 'glitter' of his public life . . . It was a vicarious existence, for without Weizmann she would have hardly enjoyed these social benefits, and so she jealously guarded his station in public affairs against all detractors, equally for her own gratification as for his" (p. 93).

Rose's verdict on Weizmann is quite damning: "Subtle, impressionable and untrustworthy, he could and would let you down. He harboured grudges and interpreted criticism as personal enmity. He used people, exploited them, and then discarded them, not so much out of spite as for reasons of expediency, *raison d'etat*" (p. 263). Perhaps the bitterness of his old age was in part retribution.

In 1937 British sponsorship of Zionism fell victim to Neville Chamberlain's (and later Churchill's) appeasement of the Arabs—the story is well known already. Weizmann himself, whose entry into British high circles was his "special province, his trump card" was now fatally compromised (p. 349). Jan Masaryk, the Czech minister in London, advised Weizmann to "buy a house with three stories: the first for Heile Selassie, the second for Masaryk, and the third for Weizmann himself" (p. 339). When in May 1945 Weizmann wrote asking Churchill to make good his promises "to eliminate the White Paper, to open the doors of Palestine, and to proclaim the Jewish State," the prime minister "fobbed him off" (p. 402).

On May 16, 1948, Weizmann was elected president of the new state of Israel. After the initial euphoria he was unable to reconcile himself to the restraints of a symbolic, ceremonial presidency.

Weizmann's last years were soured by humiliation, bitterness, and self-delusion. Appropriately, Weizmann's own *shtetl* humor is quoted to describe his feelings. When someone retrieved a dropped handkerchief, Weizmann quipped "nowadays, this handkerchief is the only thing I am allowed to poke my nose into" (p. 445).

Shadlanim (pp. 142, 514) should be *shtadlanim*. It would be easier on the reader if the footnotes had supplied dates for the Weizmann letters.

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MOSHE DAVIS, editor. *With Eyes toward Zion*. Volume 2, *Themes and Sources in the Archives of the United States, Great Britain, Turkey, and Israel*. (Second International Scholars Colloquium on America–Holy Land Studies; America–Holy Land Studies.) New York: Praeger; distributed by Greenwood, Westport, Conn. 1986. Pp. xxvi, 408. $42.95.

The facts that the terms "Jerusalem" and "Zion" were in the past frequently used as metaphors for the New World and that American place-names sometimes read like an atlas drawn from the Old Testament are evidence enough of the image of the Holy Land in the American conscience. Clearly, Bible consciousness, which persists in Protestant Evangelicalism and is also found in the Mormon analogy and the spirituals of the black church, places the American relationship with contemporary Israel somewhere beyond the normal.

Drawn from papers presented at the Second International Scholars Colloquium on American–Holy Land Studies held in 1983, this collection of essays gives the reader an indication of the wealth of archival material now available in repositories in Britain, Israel, and Turkey. The collection is anchored at either end by convincing essays and comments that reinforce the impression that Holy Land studies has finally emerged as an important historical subspecialty. Its existence is due in no small measure to Moshe Davis, who first saw the need to establish "a unified field in which American history meets the Jewish and Christian traditions" (p. xxii).

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His long-range goal is to create a specialized American Holy Land documentary library for scholars. He is well on the way to doing so. In 1977 Arno Press published reprints of seventy-one of the best-known books dealing with the theme of the Holy Land. With the cooperation of the American Jewish Historical Society, four volumes of the *Guide to American Holy-Land Studies, 1620–1948* have already been published, and more are on the way. In addition there is the well-received collection edited by Robert T. Handy, which deals with the Holy Land theme in American Protestant life. That hardly exhausts the list, but it is sufficient to demonstrate what the driving energy of Davis can do when it is unleashed.

Although the papers that form the heart of this book present an enticing glimpse of the material available in the aforementioned archives, it soon becomes clear that the Holy Land poses special problems for the researcher. As Ruth Kark observes, quoting from a report of the American consul J. G. Wilson, in one of the most instructive papers in the collection, "the imagination misleads the judgment, sentiment prevails" (p. 127). These memoirs, diaries, personal correspondence, and consular reports can be used only with great care because they contain an "extraordinary amount of misinformation" precisely because they deal with a subject that is "holy" to so many. Consul Selah Merril, for example, is unable to comprehend the "return to the soil" motif in Zionism not only because the levantine people he imagines Jews to be could not possibly become farmers but also because he resists the notion that they could again farm *their* holy land.

Interestingly, we may find important new information about American Jewry in an unexpected source, the written observations of the "messengers" who visited American Jewry. Also, judging from the sheer number of journals that were published, we are again reminded how extraordinarily variegated American Jewry was in the nineteenth century and how vicarious its brand of Zionism. Concerning the latter, Michael Brown observes in an instructive essay that American Jewry was not merely another diaspora but became the antithesis of the Zionist dream. The earliest *Yordim*, the involuntary emigrants who came to these shores during World War I, understandably did not like what they saw. Men such as David Ben Gurion and Meier Berlin, one of the founders of Mizrachi, who were rescued from Turkish deprivations by American government action, were nevertheless unable to sympathize with support of the *yishuv* given by American Jewry from a distance. They hoped for actual settlement in Palestine, nothing less would do.

But American Jewry eyed Zion from a philanthropic vantage. They were proud of the elaborate philanthropic apparatus that they were developing. Philanthropy rather than settlement in Palestine became the most Judaic aspect of American Jewry, and it was central to the kind of Zionism American Jewry developed. In some measure what we see today in the United Jewish Appeal, its major philanthropic endeavor, is the assigning of a major share of the philanthropic dollar to the support of Israel. That follows from the original precedent for the distribution of *chalukah*, the support of the religious settlement in Palestine in the eighteenth and nineteenth centuries. One of the most important insights gleaned from the papers of Kark, Brown, and Menahem Kaufman is how early and crucial was the philanthropic character of American Jewry's vision of Zion. From these observers we also catch a glimpse of how ambivalent Zionist leaders who had settled in Palestine were about the corrupting effects of philanthropy, not only on the intended recipients but also on the givers, for whom it led to a proxy Zionism that was too anemic to leave a fingerprint on history. History was one of the things Zionism hoped to restore to the Jewish people.

There are many more insights to be gained from reading the instructive papers that compose this volume. One of the most important is that it becomes clear that Washington's strong ties to Israel go far beyond the machinations of a devious Zionist movement, as some would believe. If this collection helps that truth emerge it would be sufficient. That it takes the reader considerably beyond is gratifying.

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WARREN F. KIMBALL, editor. *Churchill and Roosevelt: The Complete Correspondence*. Volume 1, *Alliance Emerging: October 1933–November 1942*; volume 2, *Alliance Forged: November 1942–February 1944*; volume 3, *Alliance Declining: February 1944–April 1945*. Princeton: Princeton University Press. 1984. Pp. clxiv, 674; 773; 742. $150.00 the set.

These volumes contain the most complete collection of messages between Churchill and Roosevelt ever published. Warren F. Kimball has tracked down documents on both sides of the Atlantic and has included telegrams, letters, book inscriptions, several indirect exchanges through third parties, two German intercepts of telephone messages, some information from MAGIC and ULTRA sources, drafts, and messages not sent. Also included are the initials of the person who drafted Roosevelt's messages, and the specific changes made by the president. This helps clarify which ideas came from Roosevelt and which originated from his staff members. Previous printings or partial printings of the

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documents are noted, and errors in dating have been corrected. The editor provides details on how the messages were sent and the files in which they were kept. The documents are preceded by headnotes that set them in context, identify persons, places, and things referred to, and cite pertinent books. Kimball uses the headnotes and the essays on various topics as a means of presenting "a broad and comprehensive interpretation of the politics of strategy in World War II, particularly as viewed by Churchill and Roosevelt" (vol. 1, p. xvii). In addition, Kimball has chosen most of the pictures with a view to expressing the mood and atmosphere of the meetings between the two leaders.

All the great and minor issues, disagreements, moments of joy and discouragement, troublesome personalities, periods of decision and indecision come alive again as one reads these wonderfully candid exchanges. Presented here are the problems in France both before and after its defeat in 1940. Through Churchill's words we recapture the anxiety and drama of Britain's struggle to survive Hitler's onslaught. After June 1941 Stalin and the situation in Eastern Europe were seldom out of the minds of Churchill and Roosevelt as they discussed the allocation of military supplies, the battle of the Atlantic, the when and where of the second front, what should be done to accelerate the downfall of the Axis, and the governance of liberated territories. The two leaders also coped with the relationships of the campaigns in the Mediterranean, the Pacific, Southeast Asia, and the Balkans to the overall strategy. Then, too, there are the matters relating to the development of the atomic bomb and to the political and economic structure of the postwar world. If these volumes contain no great surprises for scholars of military and diplomatic history, they do add nuances and enrich our understanding of these years.

In view of the time and effort that have gone into the compilation and review of these volumes, it is too bad that some flaws were not detected. Early in January 1941 Churchill sent Roosevelt his good wishes "for tomorrow's momentous statement" (vol. 1, p. 126). Here a note would have been appropriate to the effect that on January 6 the president addressed Congress and called for all-out aid to the democracies fighting Hitler. This led to the Lend-Lease Act. In the same speech Roosevelt set forth his vision of a postwar world founded on the Four Freedoms. These were freedom of speech, religion, and from fear and want. He said that these principles were a definite basis for a world that was attainable within their generation. Roosevelt spoke about the Four Freedoms again on March 14, at a dinner of the White House Correspondent's Association. Kimball has a note on that dinner but nothing about the Four Freedoms. Also, nothing is noted about Roosevelt's State of the Union message of January 6, 1942, delivered during the Arcadia conference, in which he said that the war objectives of the United States were to smash militarism, liberate subjugated nations, and establish and secure the Four Freedoms everywhere in the world. As late as August 25, 1943, in a speech before the Canadian Parliament, Roosevelt was still arguing that the principles enunciated in the Atlantic Charter and the Four Freedoms were attainable. Consequently, the Four Freedoms were emphasized in American domestic propaganda throughout most of World War II. Such an emphasis raised public expectations and brought embarrassment to the president later, when reality failed to match the promises. But in these volumes the Four Freedoms are mentioned only in a December 1943 letter of Brigadier General Patrick Hurley to Roosevelt, and they are not defined. From time to time there are references in the documents or the notes to Roosevelt's concern about public opinion, but nothing seems to have been consulted that would give the reader any sense of the nature, causes, and dimensions of such sentiments.

On the matter of books, an earlier, selective, one-volume compilation of Roosevelt-Churchill messages that appears in many footnotes under the initials "R & C" is incorrectly cited in the introduction and is not listed in the bibliography. It is surprising to find no reference to Charles Bohlen's memoirs in the discussions of Teheran and Yalta, and it is missing from the bibliography.

There is also a lack of uniformity in the notes on the ranks of military and naval personages and in the identification of objects. More than forty years have passed since World War II, and many a younger reader may wonder what the difference was between a light and a medium tank, or what is meant by such terms as a pocket battleship, Ju 88's, A-20's, Boston Bombers, and Enfield rifles, among other things. But when all is said and done, the volumes represent a notable achievement and will be welcomed by the scholarly audience for whom the work is intended.

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FIONA VENN. *Oil Diplomacy in the Twentieth Century.* (The Making of the Twentieth Century.) New York: St. Martin's. 1986. Pp. xii, 228. $29.95.

Fiona Venn defines oil diplomacy as "the role of oil in international relations, both as a factor directly influencing intergovernment relationships, and as a multinational enterprise" that affects the policies of the states where it operates (p. 9).

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Prior to World War I, oil served diplomatic interests rather than vice versa, she notes. But suspicions arose in Britain that Standard Oil influenced U.S. policy, and the Americans became convinced that petroleum significantly affected British diplomacy.

The search for reserves to fuel oil-fired fleets soon provoked greater government involvement. Dutch policies strengthened the Americans' concern that they were being excluded from development of colonial resources by strategy orchestrated in Europe. In a soundly developed chapter, Venn shows how the United States became more active in the interwar years, used the open door to enter the Middle East, and then slammed shut the door. She believes the British displayed forbearance, admitting U.S. interests into the region out of concern for broader Anglo-American relations. By the close of the period, there were closer relationships between oil companies and their governments, nationalist movements in many nations saw the industry as a threat to political and economic sovereignty, and the parent governments of the companies were recognizing their inability to prevent expropriations or establishment of state monopolies.

World War II stimulated American interest in developing foreign supplies rather than exhausting domestic reserves. The ARAMCO partnership "marked the final rejection of ideas of actual government participation in the oil industry" and a shift to the use of private interests to serve public policy (p. 111). Venn well demonstrates the role of independent oil companies in breaking the price-control hold of the major companies and describes how Colonel Muammar el-Qaddafi transformed the role of oil in international relations. She argues that, Yom Kippur War or not, in 1973 OPEC would have forced increases in oil prices. The use of oil as a political weapon by the Arab countries at that time, however, "demonstrated categorically that the oil companies would not necessarily support their parent governments when faced with a conflict of interests" (p. 13). Venn criticizes belligerent U.S. policies in the years after 1970, which she believes have undermined the more patient and conciliatory diplomacy of European nations.

The wide-ranging discussion provides a useful survey. More information on interaction between companies and governments would be helpful. So, too, would greater clarity regarding the distinctive features of "oil diplomacy" as compared with those of other types of diplomacy.

JONATHAN E. HELMREICH
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JEFFREY RICHARDS and JOHN M. MACKENZIE. *The Railway Station: A Social History*. New York: Oxford University Press. 1986. Pp. xix, 440. $22.50.

When in 1900 the Boxers rose in China, one of the first things they did was to wreck railway stations and destroy the tickets, "for, said they, if there are no tickets, people will no longer be able to travel" (p. 254). More than missionary churches, railway travel represented the intrusive West.

This rich book makes very clear that, for a century, modernity and railroads marched hand in hand. That much we know, although Jeffrey Richards and John M. MacKenzie provide illustrations and anecdotes to enliven the familiar theme. But they also insist on the extent to which the worldwide spread of railways matched the spread of Europe's economic and political dominion, and there much of what they say is new. Focusing less on railroads as such than on railway stations, they show first England, then Europe, then the rest of the world, covered with the nineteenth-century counterpart of Raoul Glaber's white mantle of churches: the stations that they regard as "the distinctive architectural form of the late nineteenth and early twentieth centuries," also "the first which was genuinely world-wide" (p. 67).

Based on secondary reading, the book is compendious, uneven, yet frequently fascinating. It seems to be addressed to the general reader, although specialists will find it a treasure-trove of ideas and anecdotes about a phenomenon that lasted as briefly as Europe's world dominion. They will also find, as I did, minor errors (Watts is not a county of Los Angeles; Washington is not a baroque capital) and questionable views: the Beaux Arts style may well reflect the megalomania of American railroad companies, but then what about the prodigal Victorian Gothic of St. Pancras, let alone the exuberance of Bombay's Victoria Terminal? Finally, there are bound to be omissions. To each his own. I would point out that, although much is said of station architectures, no mention is made of their influence on civilian styles, especially the most humble, as in the countryside of France or Central Europe.

That being said, most of the book is fun to read. It begins with three lavish chapters on station architecture. These rehearse all possible styles from classical (New York's Grand Central inspired by the baths of Caracalla), through Oriental, Romanesque, Gothic, Renaissance, Baroque, and Beaux Arts, not forgetting the humble shed, the Swiss chalet, and the fortified castle (as in India after the Mutiny). Other chapters discuss the station and society (the triumph of standard "railway" time, of timetables and punctuality, the rise of railway town and of suburbia, the place of stations in town planning); stations and politics (essential to communications, postal services, mass-circulation newspapers, not to mention whistle-stop tours launched by William Gladstone in the 1870s and put to bed after Harry Truman's 1948 electoral campaign); the station in

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the economy (what you might expect, but also milk trains, fish and fresh produce shipments, the gobbling up of forests in some areas and the creation of others elsewhere, all for fuel); stations in wartime (raided, besieged, wrecked, rebuilt, and busier than ever); stations offering facilities not only to travelers but to the community around (bookstalls, buffets, "temples of convenience," and the rest); stations appearing in literature and film (from Jacques Offenbach's *Vie Parisienne*, which opens at the Gare de l'Ouest, to David Lean's *Brief Encounter* and beyond); and class, race, and sex (cheap workmen's trains, ladies' lavatories, women's work, and drunken engine drivers). On this generous menu every reader can find something to tickle the palate.

In the end, though, Richards and MacKenzie's book is a nostalgic salute to lost glories. Killed by cars, trucks, and airplanes, stations are ready for museification, when they are not actually turned into museums—most recently at the Gare d'Orsay in Paris, opened as late as 1900. The National Endowment for the Arts issues booklets on the reuse of railway stations. Some have been turned into private homes; others serve as banks (Lincoln, Nebraska), casinos (Adelaide, Australia), cultural centers (Duluth, Minnesota), supermarkets (Windermere), restaurants, boathouses, and pubs. *C'est magnifique, mais ce n'est pas la gare.*

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ANCIENT

WILLIAM A. WARD. *Essays on Feminine Titles of the Middle Kingdom and Related Subjects*. Beirut: American University of Beirut; distributed by Syracuse University Press. 1986. Pp. xvii, 198. $30.00.

A dearth of records exists for everyday life in most ancient cultures, and Egypt is no exception. Even more scarce is explicit information on the roles that women played in that society. All the more reason to welcome the present study on feminine titles from the Middle Kingdom (ca. 2050–1650 B.C.). A companion volume to the author's *Index of Egyptian Administrative and Religious Titles of the Middle Kingdom* (1982), the present collection of essays has two basic objectives: to attempt to define the relative social status of some women on the basis of their titles and to show that harems and concubinage did not exist in the Middle Kingdom.

William A. Ward has gathered lists of nonroyal feminine titles, from which he draws a number of pertinent conclusions; for example, the temple personnel was largely male, and on large private estates men generally seem to have worked outside, whereas women worked inside. His study of royal titles used by commoners (such as "king's daughter") shows that this practice only occurred at the end of the period studied, when the crown's authority was declining and it was granting special favors to gain further support. Other titles such as "hereditary noblewoman" simply showed the individual's status in her community.

The second major section opens with remarks on monogamy and the legal status of women. Ward shows that the norm was monogamy but that no laws prohibited polygamy; it would, in fact, seem that polygamy was limited to individuals who were financially able to support more than one family. As a general principle, men and women were equal before the law, and, theoretically at least, economic or social standing conferred no preferential status. Next, the study of the various terms said to mean "harem-woman" demonstrates that some of the so-called concubines were actually married women and that others were men. The translation "concubine" is thus effectively negated in all cases, and Ward offers more appropriate translations for the various terms.

The book ends with a list of useful indexes given both in transliteration and in translation. The latter will obviously make the book accessible to nonspecialists, and for this the author should be thanked. Given that the book seems to have been written with a wider audience in mind, however, the inclusion of a larger selection of secondary sources in the notes would have let his readers know how much more work has recently been done on some of the topics discussed. This observation should not detract from Ward's achievement. The subject is a difficult one, given the paucity of material extant, and Ward's rigorous methodology has handled the problems well. Such scholarship will surely inspire further research.

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MORRIS SILVER. *Economic Structures of the Ancient Near East*. Totowa, N.J.: Barnes and Noble. 1985. Pp. 211. $28.50.

Written sources for the economic and social history of ancient Mesopotamia, in sheer bulk at least, dwarf those of the rest of the ancient world. Whereas Assyriologists have labored gamely over this mountain of material for the past century, the results of their work tend to be closely focused and often remain little known outside their discipline. The result has been that few historians of ancient economy know anything in detail about Mesopotamian evidence and that, when they deal with the

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ancient Near East, they tend to set forth more theory than useful information.

Morris Silver is well aware of these problems and offers what is in many respects a pioneering enterprise. His book has two main purposes. One is to demolish the hypotheses of Karl Polanyi, who proposed that ancient Mesopotamia, not to mention the rest of the ancient Near East, lacked markets for products and consumer goods. Polanyi's work is not widely known among Assyriologists, although some have consulted his volume *Trade and Market in the Early Empires* (1957). Thus, Silver's polemic against Polanyi will prove of more interest to economic historians than to Orientalists, few of whom will need to be convinced that Polanyi's thesis is without merit.

The real interest of Silver's book to the general historian lies in its demonstration that modern economic theory can be applied successfully to ancient Near Eastern evidence. Taking as its point of departure "transaction costs," Silver's survey offers numerous valuable and stimulating analyses of ancient Mesopotamian economy. His remarks on the "temple economy," "gift trade," the role of women and family structure in commerce, "community ownership," transport costs, interest rates, to name a few topics, are both original and important and should stimulate continued, fruitful investigation of the sources.

Silver has read assiduously in the technical literature of ancient Near Eastern studies and has carried on a vigorous correspondence with several specialists. This has given him a grasp of the evidence and the controversies surrounding its interpretation that removes his work from the level of purely theoretical discourse and reassures the specialist reader. This is not to say that important publications have not been overlooked or that obsolete ones have not, unfortunately, been used here and there. Nor will every reader feel comfortable with Silver's occasionally dizzying juxtapositions of data from different periods and places. But his work is firmly grounded in the known and is filled with interesting suggestions and new insights.

Silver earns the reader's gratitude by writing clearly and avoiding jargon (one pauses over "productive factor endowments" [p. 67]). He explains his terms well. The "economically naive" (p. 141) can read this study with ease and understanding, yet economic historians will find the level of sophistication they expect from a colleague and that is absent in Near Eastern specialist literature on the topics Silver discusses.

Every scholar interested in the economy of ancient Mesopotamia should read this book. It shows how much a measured dose of theory can help the decipherer of evidence who opines that the words in the sources alone will tell all that can be known. At the same time it proves that economic theory alone, when sources are available, is liable to be barren and misleading.

BENJAMIN R. FOSTER
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ALAIN BALLABRIGA. *Le Soleil et le Tartare: L'image mythique du monde en Grèce archaïque.* (Recherches d'histoire et de sciences sociales, number 20.) Paris: L'Ecole des Hautes Etudes en Sciences Sociales. 1986. Pp. 298. 230 fr.

In a comprehensive and compelling exercise in intellectual history, Alain Ballabriga seeks to recover the Greek geographic or, rather, cosmographic world view in the archaic period. He does this by a series of detailed investigations into relevant passages in Greek literature, especially Homer, Hesiod, and Stesichorus, and also draws on evidence from later authors, including Herodotus and Thucydides, the tragedians, and such geographical authorities as Strabo and Pausanias. Ballabriga commands modern secondary literature, from technical textual commentaries on Greek authors to intellectual, social, and religious histories by R. B. Onians, Marcel Detienne, Jean-Pierre Vernant, Pierre Vidal-Naquet, and Mircea Eliade.

The underlying arguments (most directly formulated in the section "Results and Generalizations" [pp. 59–74]) are as follows: modern scholars generally exaggerate the state of geographical knowledge among the Ionian philosopher-scientists of the archaic period. Concomitantly, knowledge attained only in Hellenistic times has been unwarrantedly assumed for earlier periods as well. Further, Hellenistic expertise in scientific geography has itself also been overrated. But, even assuming a relatively high level of knowledge of geography in the archaic era, that knowledge would have belonged to a coterie of specialists without affecting the beliefs and opinions of the mass of people. For insight into the latter, we must therefore turn to mythopoetic rather than protoscientific sources.

The book has four chapters: "Imaginary Cosmology and Archaic Geography," "Variations on the Coincidence of Opposites at the Western and Eastern Boundaries of the Universe," "The Solstitial Horizon," and "The Universe and the Abyss." Numerous subdivisions are devoted to description and discussion of passages apparently presenting knotty geographical problems. Examples range from the locating of Bronze Age Pylos in the western Peloponnese (no less than three possible sites) to the ambiguities surrounding the "double Ethiopia" and a host of geographical and cosmographic puzzles in *Odyssey* 9–12 and Hesiod's *Theogony*. Throughout, Ballabriga shows that we must not apply the catego-

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ries of scientific geography to issues of another order of thought. As he puts it, in reconstructing the archaic Greek view of the world, we have to do with a *carte-image* rather than a *carte-instrument* (see George Kish's *La carte, image des civilisations* [1980]). The *carte-instrument* is an actual graphic rendering of a specific, precisely delimited, and relatively familiar area marked by natural boundaries. The *carte-image*, by contrast, is a notional model of the universe as a whole, fixing in space the (often imaginary) regions beyond the horizon, in this case, beyond the outer limits of the Aegean world. The orientation of those regions and the nature of the frontier between the familiar and the fabulous are Ballabriga's principal interests. The result is a useful elucidation of Greek ideas about such phenomena as *axis mundi*, the path of the sun and its night course, and the location of the underworld in the scheme of the natural order, among many others. The book is also a forcefully persuasive assertion of the distinctive and overarching value of mythopoetic evidence in the history of ideas.

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K. ADSHEAD. *Politics of the Archaic Peloponnese: The Transition from Archaic to Classical Politics*. Amersham, U.K.: Avebury; distributed by Gower, Brookfield, Vt. 1986. Pp. 142. $24.95.

The starting point of this small volume is the contention that a network of roads unified, geographically, the northeast Peloponnesus. In the archaic period this geographical unity presented an opportunity for political unification; at the same time, however, political differences between the city-states situated in the area worked against political unification. Political differences are, for K. Adshead, synonymous with religious differences, for archaic politics was dominated by religion. Throughout the fifth century B.C. the old politics of religion gradually gave way to the new, more secularized politics of the classical period; in the transition from the one to the other the years 480–460 B.C. were crucial. In these decades, Corinth, in response to the growth of Athenian naval power, attempted to unify the area, or at least its northern reaches, by using the old politics of religion, as reflected in the Corinthian takeover of the Nemean games and the change from a crown of pine to a crown of celery at the Isthmian games. The Corinthians failed, in part because they "seem to have been disinclined to maintain the Dorian mode" (p. 76), and in part because at precisely this time a wave of new politics was sweeping over the region. Behind this wave was Themistocles, who in the brief space of two years, 471–469 B.C., was instrumental in bringing democracy to Argos, Elis, Mantinea, and Tegea, creating in the process a solid anti-Spartan, anti-Corinthian bloc. With Themistocles' departure from Argos, however, support for the new politics quickly evaporated, and the Argives were thus no more successful than the Corinthians had been.

Adshead has read widely and taken many of his ideas from modern historical literature as well as the literature of geography, religion, and anthropology. He is familiar with a variety of ancient evidence drawn from the literary sources, myth, religion, epigraphy, numismatics, and archaeology. Yet the ancient evidence provides little direct support for many of the arguments presented here. It is, in fact, so vague and incomplete that it can be interpreted at will. No interpretation thus far advanced has pleased everyone, and Adshead's effort is not likely to alter that condition. Nor is the case presented as lucidly as it might have been. The meaning of phrases such as Dorian mode, Dorian spirit, and "hieratic obscurantist world order" (p. 103), to mention just a few, is by no means clear, and this lack of clarity is exacerbated by a penchant for lengthy sentences.

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L. B. CARTER. *The Quiet Athenian*. New York: Clarendon Press of Oxford University Press. 1986. Pp. xi, 211. $45.00.

In his preface L. B. Carter says he wanted to write about classical Athenians who "were out of tune with the democracy" and organized his book through an exhaustive study of the word *apragmosyne*, translated as "quietism" (p. vii). Chapter 1 sets out to show what the quietist reacted against. Carter maintains that "the pursuit of fame and honour was the foremost preoccupation of any Greek who could afford it" (p. 2). The chapter ends by stating the book's object as the examination of the countermovement, *apragmosyne*, or "minding one's own business." Carter looks, then, for persons who disapproved of the Athenian democracy, its empire, and the continuing tradition of striving for fame and honor.

Chapter 2 begins by asking whom Pericles had in mind when he referred to an *apragmon* in his Funeral Oration and concludes by suggesting three broad groups, discussed further in chapters 3–5: a disgruntled upper class, peasant farmers, and "rich quietists." Chapter 3 identifies a group of "noble youths" who disliked democratic politics, opposed the empire's expansion, and gradually became more active. Carter credits this group with the mutilation of the Herms, which he terms an "organized demonstration" against the Sicilian expedition (p. 74); he blames the same men for the oligarchical revolu-

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tions of 411 and 404 (pp. 74, 194). Chapter 4 emphasizes the small turnout at assembly meetings. Among the "rich quietists" discussed in chapter 5 Carter includes Nicias, perhaps the most prominent politician of his day, two other generals, Lamachus and Laches, and those rich men who made lavish public expenditures but tried to avoid the courts. I see little evidence in these chapters to show that either rich or poor opposed the empire and traditional competitive values.

Chapters 6 and 7 seek to establish that the pre-Socratics did not practice unworldly contemplation and that Plato did, influenced by the social and political circumstances of his day.

In the concluding chapter Carter says that "the central importance of a study of *apragmosyne* lies in correcting our overall vision of the democracy" (p. 193). He proposes, however, only corrections already well known and better treated elsewhere. Simple errors mar the book. Peasants did not accept oligarchy in 411 because they wanted peace (pp. 97, 104; see Thucydides 8.53–54). The Sicilian expedition did not have only 8,300 men (p. 79; see Thucydides 6.43). Carter fails to move beyond collecting occurrences of *apragmosyne* to exploring significant questions: Is H. T. Wade-Gery's picture of high-society, conservative "internationalists" who opposed the radical democracy and its empire correct? (Carter notes but does not address A. Andrewes's criticisms in the *Journal of Hellenic Studies*, 1978.) How did any "quietist" ideals such men might have had affect the revolutions of 411 and 404?

PETER KRENTZ
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HANS-JOACHIM GEHRKE. *Jenseits von Athen und Sparta: Das Dritte Griechenland und seine Staatenwelt*. Munich: C. H. Beck. 1986. Pp. 216. DM 32.

Written in a clear, flexible style, this book concentrates on the Greek city-states that were usually smaller and less influential than Athens and Sparta. It limits its scope geographically to Greece proper, the Aegean, and Asia Minor, thus omitting the western Greek colonies, and temporally to the years from the Archaic period to the Roman conquest. The emphasis is to be warmly welcomed, for Hans-Joachim Gehrke rightly observes that no genuinely valuable history of Greece can be written without a proper appreciation of this wider Greek world.

The contents of the book are broader than its title indicates. Part 2 treats the common cultural environment of the Greeks in a section that forms the necessary political, social, and economic background of the later discussion of individual states, while providing the justification for a non-Aristotelian classification of them. The author starts appropriately enough with the geographical features of the area and their impact on economic life and social structure. Into this setting he places a discussion of the development of the polis, its history, and its political divisions. Although admitting that they are not always rigorously distinct, he creates three broad categories for political and, by extension, for military conflict: the internal conflict of local groups within a polis; "international conflicts," here defined as efforts to unify a region, such as the *synoikismos* of Elis around 472; and "integral conflicts," or larger wars involving a great number of city-states, as exemplified by the hegemonial struggles of the fourth century. From politics the author proceeds to culture, which includes such diverse topics as the diffusion of Greek dialects and the scripts in which they were written, religion and myth, and, lastly, literature, philosophy, and art.

In Part 3 the author arranges the Greek states in six types primarily on the basis of economic criteria. First come the most significant agrarian states, notably those in Thessaly and Boiotia and then such lesser counterparts as Phleious and Argos (type 2), the latter of which could far more reasonably be included among the greatest agrarian states. Then follows a gradation of medium-sized agrarian states with significant maritime interests (type 3), such as Rhodes and Corinth, similar smaller poleis such as Sikyon and various Aegean islands (type 4), and poorer states such as Phokis (type 5). Type 6, which embraces states with special individual features, includes Paros because of its mineral wealth, Delphoi and Delos because of their religious import, and lastly Aigina and Phokaia because of their commercial interests.

The author's typology, which is useful more for description than definition, admits of no fast criteria, as the example of Argos demonstrates. Further, the author himself observes that socioeconomic factors more accurately describe the degree of quantitative development than its qualitative nature. One can, moreover, argue against the author's contention that the national consciousness of the Greeks was essentially apolitical (p. 11). The very multiplicity of political forms argues in favor of a vast, varied, and intense interest in the polis and its working. Within these boundaries, the chief value of this book is the fresh perspective that it gives to the world of the polis in its heyday.

JOHN BUCKLER
University of Illinois

FRANÇOISE-HÉLÈNE MASSA-PAIRAULT. *Recherches sur l'art et l'artisanat étrusco-italiques à l'époque hellénistique*. Foreword by MARIO TORELLI. (Bibliothèque des Écoles Françaises d'Athènes et de Rome, number

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257.) Rome: Ecole Française de Rome. 1985. Pp. xvii, 274.

The fatalistic character of the Etruscan world view is described by Seneca: "The difference between us [the Romans] and the Etruscans is this: while we believe that lightning is released as a result of the collision of clouds, they believe that clouds collide in order to cause lightning. For since they attribute everything to the gods' will, they believe, not that things have a meaning insofar as they occur, but rather that they happen because they must have a meaning" (*Natural Questions* 32.2). In their view of history the Etruscan people had been allotted a limited number of *saecula*, at the end of which period the Etruscan "name" would disappear. In the last few years the appearance of surveys of Etruscan history by K. W. Weeber, Mario Torelli, and Michael Grant has provided a modern pattern of their internal political development and of their international relations. Details are hard to recapture, for, although the Etruscans were the most literate people in Europe after the Greeks (thirteen thousand inscriptions have come down to us so far, ranging in date from the seventh to the first century B.C.), they left no literature. A picture of their history must derive from archaeological, artistic, epigraphic, and linguistic evidence, supplemented by scattered references in Greek and Roman writings.

Their art, on which we depend for evidence, must be read historically; at the same time, it must be placed in a historical context. In the present book Françoise-Hélène Massa-Pairault attempts to do this, following the social approach to the history of art identified in Italy with the work of Ranuccio Bianchi Bandinelli, whose influence has deeply marked the present generation of Etruscan and Roman scholars. As Bianchi Bandinelli pointed out, many aspects of Etruscan culture can be explained by the persistently aristocratic nature of its society from the eighth century B.C. on. Artisans depended not only on models from abroad but also on the tastes and requirements of their patrons. The subject of the present work is the relationship of the work of art to its patrons and the reflection of a certain social and political ideology in its iconography.

The chronological range is broader than the title would imply. Part 1 examines the role of the artisan in the developing Etruscan city, using the example of archaic Rome as a likely parallel. An illustration of the results to be obtained from an iconographical study is provided by an examination of the mythological compositions of the architectural decorations from the temples at Pyrgi, the ancient port of Cerveteri. The subject matter of the earliest temple is seen as signifying the archaic tradition of the monarchy, with the regular course of the dawn and the sun as the symbol of the stable rhythm, activity, and immortality of the king; this symbolism reflects the contemporary Greek philosophical idea of the close relation between the *physike* and the *politike*. The terra-cotta decoration of the later Temple A (the Seven against Thebes) would instead refer symbolically to conflicts between aristocratic families, such as those that exploded at the end of the sixth century B.C. in Etruria.

The model for such an understanding of the Etruscan transformation of Greek myth, based on the Etruscan view of history expressed above, is provided by a monument examined in part 2. The "François Tomb" at Vulci, family tomb of Vel Saties, an Etruscan general who celebrated a triumph over the Romans during the bitter fighting in the fourth century B.C., was decorated with wall paintings picturing various moments of history intimately connected in the Etruscan vision of time. A scene from Greek mythology (Achilles' sacrifice of the Trojan prisoners) faced, as an obvious parallel, a scene from the early history of Vulci. Related to this heroic past is the heroic present shown by the portrait of Vel Saties in his triumphal costume; his reading of the bird omens refers to the future. Other parallels symbolically join figures from Greek mythology with members of the Saties family. As Filippo Coarelli points out, details of the interpretation may vary, but the method is sound (Coarelli, in *Dialoghi di Archeologia* [1983]: 43–69). Massa-Pairault collects, tabulates, and discusses evidence from inscriptions, sculpture, engraved bronze mirrors, tombs, and coinage to show how the François tomb reflects the emergence of a new "middle class" alongside the older aristocracy and how the situation affected the status of craft workers and their patrons.

Part 3 deals with the last flowering of Etruscan art, craftsmanship, and "national" consciousness before the end of their autonomy, with eastern Hellenistic and Italic styles and themes, and with the Etruscans' final Romanization as reflected in the monuments, including the thousand or so decorated funerary urns of this period from Volterra and other northern Etruscan cities. The author is at her best in dealing with this material, on which she has previously done important work; some of her suggestions will surely be taken up by scholars. Unfortunately, the mass of detail, unverifiable hypotheses based on obscure literary references, and the lack of a real conclusion make the book difficult to read and limit its usefulness for readers attempting to discover how the Etruscans saw their world.

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JACQUES POUCET. *Les origines de Rome: Tradition et histoire*. (Publications des Facultés Universitaires

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Saint-Louis, number 38.) Brussels: Facultés Universitaires Saint-Louis. 1985. Pp. 360.

Jacques Poucet's thesis is that not only is the historicity of the traditions about pre-Etruscan Rome—Rome from the time of Evander through the reign of Ancus Marcius—questionable but those traditions can be shown to be false, riddled with contradictions and improbabilities, or the residue of an ancient Indo-European system of religion and government. The rejection of the historicity of Numa Pompilius and implicit acceptance of that of Servius Tullius is significant; Poucet is an ardent disciple of Georges Dumézil.

It has been observed that the destruction wrought by the Gauls in their sack of Rome at the beginning of the fourth century must have been so complete that little in the way of records or archives could have survived. Certain archaeological evidence also supports the totality of the devastation. In his preface Livy expresses serious doubts about the early traditions about the city, finding them more suited to poetic inventions than historical fact. And today very few do not believe that the early record has been heavily interpolated to serve a variety of ends. Poucet's choice of a cutoff point seems rather arbitrary; he believes in the historicity of the Etruscan domination in Rome but that everything prior to that is a fabrication of some sort.

The book is divided in half. The first half examines the actual question of historicity; the second is an analysis of the various elements and impulses that contribute to the invention of such a contrived national record. The first is divided into two chapters: "The Historicizing Approach" and "The Quest for Confirmation, the Appeal to Data outside the Tradition." The second of these is divided into subchapters on linguistics, religion, political and social institutions, ethnological comparisons, the "new comparative mythology," and archaeology. This is the heart of the argument; the rest is philosophical and literary speculation. But in a painstaking examination of each class of evidence Poucet finds nothing to confirm any substantial part of the historical tradition and much to contradict it. He is particularly interested in the Sabines and the place assigned them in the Romulean city, which he sees as completely without foundation, disproved above all by the lack of any archaeological record. He tends to stress archaeological material when it is lacking and to ridicule it when it is contradictory. He believes there was no strong Sabine element in Rome before the sixth, perhaps late sixth, century (pp. 85–86).

But the salt route attested by the Via Salaria is certainly very old. Every river of suitable size and course the length of Italy has salt pans at its mouth, and the mountain peoples used the rivers as highways to come down to the sea to get the salt. That traffic would go back at least to neolithic times. Rome's reason for existence was that it was the lowest point on the course of the Tiber at which a clumsy ferry could cross with relative safety. Within Rome the salt road ran down the ridge of the Quirinal hill and along the forum side of the Capitoline to the ferry slip at the mouth of the Cloaca brook. On the far bank of the Tiber it continued as the Via Campana/Portuensis, and the salt pans were at the river mouth. Rome's early existence was tied to the maintenance of the ferry and the sale of provisions to those coming for salt. These would have been chiefly the Sabines and their relations, the mountain people living along the left bank of the Tiber upstream from Rome. The facts that the Quirinal, down which the salt road ran, and the neighboring Viminal were called *colles* whereas all the other hills of Rome were called *montes* and that the people who lived there made up the Collina, one of the four original tribes of Rome, seem to set this area apart. That it should also have been the location of a number of shrines dedicated to gods considered Sabine by the Romans is further confirmation of this. If the Sabines were not part of the original amalgam of Rome, they must have been an early addition. Poucet needs to read Louise Adams Holland's *Janus and the Bridge* (1961).

L. RICHARDSON, JR.
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ARTHUR D. KAHN. *The Education of Julius Caesar: A Biography, A Reconstruction.* New York: Schocken. Pp. x, 514. $28.50.

It is often said that, given our evidence, it is impossible to write the biography of any Roman politician except Cicero. The problem is compounded with a book called "The Education of . . .," which ought, perhaps, to be an autobiography like its model; only so can the effects of events on a young mind be reliably estimated. Arthur D. Kahn warns us there is much speculation in this work; after a frankly imaginative picture of Caesar aged four, it turns to a detailed historical account of him and his age, in which people nonetheless scowl, blush, and (too frequently) giggle without benefit of evidence.

That is a convention that can be discounted. Kahn has read widely in both the ancient and modern literature, as many details reveal; he is aware of new work on social and economic history (less so on Roman religion, which is conventionally described as in decline). There are few actual errors, although I doubt that Roman books smelled of leather or Roman ladies knitted. Judgments on Caesar himself are mostly sensible, although we really ought to be warned that the sources are such that dispute rages

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over the interpretation of his last years and ultimate plans and cannot be finally settled.

The book is wide in scope (Caesar marries, we get an account of Roman marriage ceremonies; he goes to Rhodes, the city is described), but it lacks depth. Kahn exhibits some credulity toward ancient rhetorical historians and a desire to draw parallels between the society of the falling republic and what is seen as a similar crisis in America, which leads him to underplay the foreignness of Rome; for example, the nature of traditional societies in general and Roman ancestor worship, literal and less literal, in particular, are crucial to any understanding of the conservatism of the oligarchs. Elsewhere too Kahn is unfair to Caesar's opponents. He speaks of Pompey's "high, empty forehead," but the famous bust reveals a somewhat low forehead, and the career shows great administrative ability and an interest in and respect for intellectual matters. Cicero is treated with old-fashioned contempt, and the intellectual contrast between him and Caesar misunderstood: we are told that Caesar stood for "the enlightened Epicurean philosophy" (the idea that it was a left-wing creed has in fact been largely exploded, and Caesar seems not to have been interested in philosophy as opposed to other intellectual subjects) and that Cicero's philosophic treatises were in essence politically reactionary assaults on this doctrine. This is hardly even kindly to be described as an oversimplification.

The book, then, is not for academics; some general readers may find it a way into a fascinating period, in spite of the excess of detail that sometimes obscures the significance of events. But it fails in its ambitious purpose, for its analysis of the collapsing society of the Republic is too pedestrian to help us understand either that period or the American present.

ELIZABETH RAWSON
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ALAN F. SEGAL. *Rebecca's Children: Judaism and Christianity in the Roman World.* Cambridge: Harvard University Press. 1986. Pp. viii, 207. $20.00.

Alan F. Segal's latest work is an important contribution to the analysis of rabbinic Judaism and early Christianity from the perspectives of the social sciences. It is an effort to plumb the origins of these constellations of faith through political science, history, sociology, anthropology, economics, linguistics, and literary analysis. To this end Segal reviews the history of Israel from the Persian period to the first century. Against this background he provides detailed chapters entitled "Society in the Time of Jesus," "Jesus, the Jewish Revolutionary," "Paul, the Convert and Apostle," "Origins of the Rabbinic Movement," and "Communities in Conflict." He prefaces these with an analytic resume of the world view or "myth" that ancient Israel bequeathed to the theocracy and thence the first century. In these studies Segal applies social-scientific insights to such topics as the diminution of Near Eastern agricultural power before Alexander, the societal roots of Hellenization, the extrareligious dimensions of the Hasmonean revolution, the sociopolitical role of first-century sectarianism, the psychology of apocalypticism, the social foundations of Jesus' original support, cognitive dissidence and Pauline theology, and the social valence of the Pharisaic purity procedures.

Segal's text of fewer than two hundred pages could hardly be expected to provide a detailed history of early Christianity and early rabbinic Judaism or their social dynamics. Understandably, therefore, many facets of Segal's study require further consideration in the light of contemporary societal theory and available studies. Thus, all generic treatments of the Essenes, the Qumran sectaries, the early Christians, the emerging Pharisees, the constitutional struggles within early rabbinic Judaism, and the often specious dichotomies between the religious and the political motivation of first-century events can now be cogently reassessed on sociopolitical grounds.

Other traditionally accepted views should also be reconsidered: that the Sadducees were an upper-class group; that Paul was a convert to a new religion from the "Pharisaic movement"; that mid-first-century Judaism and Christianity were two separate religions; that the practice of the Law (written and oral) was foreign to early Christianity; that the *havurot* were "purity clubs"; that Judaism and Christianity developed into religions primarily of personal piety; and that early rabbinic Judaism and early Christianity conflicted with one another but were essentially united.

Segal's adumbration of many problems suggests the renewed youth of the fields of late biblical, intertestamental, and early rabbinic studies in the light of the approaches he represents. His brief but incisive insights into subjects such as the Passover and Gnosticism exemplify the directions needed for deeper study. Perhaps in his own elucidations the author might wish to clarify the meaning of expressions such as "intermarriage" in theocratic Israel or "national life" or "membership in the Jewish religion" for protorabbinic Judaism. Equally valuable would be an exposition of some of his novel ideas, among them that of the founding of "historical Zionism . . . by the post-exilic Isaiah" (p. 16).

Segal's work reveals a broad command of the sources and the multidisciplinary outlook requisite for modern reconstructions. He has raised issues

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that will excite discussion and respectfully challenge many a traditional approach.

MARTIN A. COHEN
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MARTA SORDI. *The Christians and the Roman Empire.* Translated by ANNABEL BEDINI. Norman: University of Oklahoma Press. 1986. Pp. 215. $22.50.

Marta Sordi has already established her reputation as an expert in the history of early Christianity. The present volume is the latest of her works translated from the Italian, which appeared in 1983. The book is divided into two unequal sections. The first, by far the longest, discusses church-state relations from New Testament times to the peace of Constantine, the second deals with issues facing Christianity in a pagan society. This section contains only a few short essays and an introduction.

The first chapter discusses the trial of Jesus and the situation of the early church up to 62 A.D. Here the author contrasts the benevolent attitude of the emperor toward Christ and the Christians with the hostility of the senatorial aristocracy. The second chapter is mostly concerned with the Neronian persecution, by which time Christianity, we are told, had penetrated the aristocratic circles of Rome; the *institutum Neronianum*, however, which the author treats as a historical fact, made Christianity into a *religio illicita*. In the third chapter we read that the Flavian emperors made the acquaintance of Christianity already in Palestine during the Jewish war, but Domitian's persecution helped identify Christianity as a form of impiety, atheism. Trajan's rescript occupies the major portion of the fourth chapter, and again Sordi claims that the emperor was completely convinced of the political harmlessness of the Christians, although Antoninus Pius had a "reactionary attitude." Marcus Aurelius actively intervened against the Christians; the main reason behind his move was the fact that the pagans mistook Montanism for Christianity. The fifth chapter deals with the Severan age, during which there was a "de facto" toleration, and the sixth with Philip the Arab, who is presented as a practicing Christian. After a brief treatment of Decius's attempted pagan restoration we come to chapter 7, which covers the reign of Valerian, whose persecution resulted in the recognition of the Christians by Gallienus. Diocletian's great persecution is the subject of chapter 8, and the first section of the book ends with chapter 9, a discussion of Constantine, who, according to the author, had a genuine and sudden experience of conversion and made "a complete break with all the traditions of the empire" (p. 135). The remaining four chapters (and an introduction) deal with the philosophical and religious traditions of Rome, the cult of the emperors, the organization of the early church, and pagan public opinion about Christianity.

Sordi knows much about the early church and the Roman empire. She also appears to be a conservative scholar who does not like to deviate from traditional Christian views. Her professed aim in this book is to prove that all persecutions were religiously motivated, because the Romans "almost never" thought of the Christians as a political threat. The point could be debated, for some of the author's arguments appear rather far-fetched. To name only two, the suggestions that Tiberius may have originated a debate in the Roman Senate on the issue of Christ's divinity is anachronistic, and, as far as Montanism is concerned, many historians refuse to classify it as a non-Christian movement and would rather think of it as a deviant form of Christianity. Unfortunately, Sordi could not consult many new studies that have recently appeared in this field in English, obviously, because of the time gap that necessarily exists between the finishing of a manuscript, its publication, and its translation into a foreign language. But, in spite of all this, she has given us a challenging book, well worth reading.

STEPHEN BENKO
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MEDIEVAL

JOHN W. BALDWIN. *The Government of Philip Augustus: Foundations of French Royal Power in the Middle Ages.* Berkeley and Los Angeles: University of California Press. 1986. Pp. xxi, 611. $50.00.

In 1923 when Louis Halphen reviewed the last of Alexander Cartellieri's four volumes on Philip Augustus, he praised and lamented "la minutie de l'exposé" in observing that the German professor at Jena had scarcely touched on the monarch's development of government and administration. Now, sixty-five years later, it is precisely that enormous lacuna that John W. Baldwin has sought to fill. In doing so, he has graciously based his narrative sections on Cartellieri's famous factual array.

Taking as his point of departure the long-held general view that Philip's reign was the turning point in the emergence of a strong and effective medieval French monarchy, Baldwin has searched for the interior *tournant*, that moment within the reign when some new royal policy set in motion the forces that accomplished that goal. In this search he has strictly rejected the earlier twentieth-century view that the maturing of Capetian government

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profited fundamentally from Philip's conquest of Normandy and Anjou whereby the more advanced administrative systems and procedures of those smaller states influenced the development of his own.

On the contrary, he insists that the *tournant* preceded the conquest of Normandy and in fact made that conquest and Philip's later victories possible. It is the central theme of his book that the turning point began with Philip's departure for the Third Crusade in 1190 when he drew up an "ordinance-testament" that established a regency and determined new procedures in certain governmental areas. The *bailli*, a little-known official before 1190, appeared with important new judicial and financial duties and with authority over the older and previously more important *prévot*. These men became the prime agents in the extension of royal justice and fiscal rights throughout the domain.

The other important innovation in the ordinance of 1190 was Philip's command that the revenues of his domain be brought to Paris three times a year where they would be audited and recorded. From this practice arose for the first time a central accounting bureau, although its earliest surviving accounts are for 1202–03. These accounts signify for Baldwin that the *tournant* had been accomplished in the preceding decade and prior to the conquest of Normandy. With the addition of territory to the royal domain during the 1190s, the accounts of 1202–03 show that Philip's revenues had increased by 72 percent since the beginning of the reign and by 50 percent since 1190. This increase gave him the enormous surplus that financed his main campaign against Normandy in 1204 and against other enemies in later years.

Included in the exposition of the *tournant* is an extended treatment of royal rights and ecclesiastical liberties. And, beyond the central theme, he has offered an absorbing essay on the emergence of royal ideology, a valuable and model analysis of the historical sources for "uncovering the government of Philip Augustus," and a generous supply of graphs, tables, and lists. When all is said, he has produced one of the most important books on medieval monarchy to appear in many years.

FRANKLIN J. PEGUES
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FRANCESCO MIGLIORINO. *Fama e infamia: Problemi della società medievale nel pensiero giuridico nei secoli XII e XIII*. Catania: Editrice Giannotta. 1985. Pp. 263.

This book is a dissertation of the "school" of legal history at the University of Catania, over which Manlio Bellomo presides. Bellomo is one of the foremost Italian legal historians, and the work of his students has his imprint: a keen interest in the social ramifications of law, combined with a superb mastery of the complicated textual tradition of medieval legal works. With the help of legal sources Bellomo and his students have made significant contributions to our understanding of the Middle Ages.

"Infamia" is a juridical term whose history begins in the ancient world and whose meaning was still debated in the nineteenth century. Francesco Migliorino has written a comprehensive study of the term in the thought of Roman and canon lawyers, with an introductory chapter devoted to the views of nineteenth-century pandectists, Christian Friedrich Glück, Karl Friedrich von Savigny, and others. There have been a number of earlier studies of "infamia." Migliorino's contribution is to look at "fama" together with its more familiar obverse. "Fama" is much more difficult to define as a term of law, for the lawyers were much more interested in the legal consequences of "infamia." He offers the helpful definition that "fama" was a means of bringing about conformity in society; "infamia" was a social sanction for diversity or "marginalization" (*emarginazione*; terms from social science do not beautify English or Italian). And he points out that definitions depended on audiences: Cino da Pistoia, fourteenth-century jurist and poet, observed that the captain of the people was "like a whore who sold herself in a whorehouse," while in his home, Lucca, he was "considered sage, a wise thief among thieves."

Migliorino demonstrates that "infamia" was much more than a social sanction in medieval law. If a person were declared "infamis" through public opinion or judgment, the legal consequences could be great. Particularly after the introduction of torture into judicial procedure during the thirteenth century, a man who was "infamis" paid the price of his infamy on the rack.

Migliorino's presentation is thorough and judicious. In a valuable appendix he has edited texts on "infamia" from eight Roman lawyers.

KENNETH PENNINGTON
Syracuse University

DONALD E. QUELLER. *The Venetian Patriciate: Reality versus Myth*. Urbana: University of Illinois Press. 1986. Pp. xiii, 386. $29.95.

In an avowedly one-sided book, written with gusto and supported by massive archival evidence, Donald E. Queller strives to dispel confusion between the "myth of Venice" and the historical reality of Venetian government in the late Middle Ages. As he rightly argues, Venetian legislation, itself confused and often ill-enforced, set members of the city's

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ruling order goals of self-effacing and self-sacrificial conduct that many of them failed to attain. Doubtless, though, they behaved rather better for having these objectives set before their eyes. Biographies of a few distinguished patricians can scarcely offer an accurate guide to the conduct of all those who shared their rank. To a large extent, the Venetian system of officeholding was designed not for the service of the state but as a form of outdoor relief for a numerous and heavily impoverished nobility. Queller's work is an encyclopedia of patricians' misconduct in their political life and provides a useful complement to Guido Ruggiero's account of noble crimes of violence toward commoners and toward each other. Queller deals concretely with such matters as electoral corruption, the formation of factions designed to control office, bribery, evasion of responsibility, dereliction of military and naval duties, nonpayment of taxes, betrayal of state secrets, and perversion of the course of justice.

Queller's method, though systematic, is literary and anecdotal rather than statistical. Example can be piled a shade remorselessly on example, conveying little sense of historical development. However, this may be an unavoidable consequence of the thesis that corruption and misconduct were persistent and timeless blemishes on the face of Venice. Ungraciously, one may at times suspect the author of embarking on a rather simple demolition job, since several of those whom he censures for undue deference to the "myth of Venice" are the authors of popular books, and others (who include Charles Yriarte, Charles Diehl, Horatio Brown, and Pompeo Molmenti) flourished in the late nineteenth and early twentieth centuries. Queller does, however, muster just enough evidence to show that, despite the iconoclastic efforts of Giorgio Cracco, the myth still exerts its power over some reputable modern scholars.

A more serious problem lies in the difficulty of defining a significant level of corruption. It is hard, in the absence of comparative evidence, to evaluate the contention that the nobility of Venice were no better, and perhaps even a trifle worse, than were other city patriciates. In an appendix Queller records, *inter alia*, 175 convictions of noblemen for offenses that fall within the scope of his survey within the period from 1450 to 1499—an average of three to four condemnations a year, including about one annual case of corruption. Given that by the early sixteenth century there were over twenty-five hundred adult male nobles entitled to sit in the Great Council, this is not self-evidently shocking. The figures are evidently incomplete, however, and the relationship between crimes condemned and crimes committed must always remain uncertain; there may have been some truth in those numerous legislators' preambles that complained, not wholly rhetorically, of widespread abuses in the Venetian nobility's far-from-disciplined ranks.

Queller's is not quite the conclusive study he might like it to be, but he offers a salutary reminder that the Venetian ruling order was neither superhuman nor in all respects unique. One remembers the rumors that circulated in 1485, after the unseemly disputes between the "long" and the "short" families over the election of Doge Agostino Barbarigo, to the effect that for all their talk of concord and unanimity the Venetians were really plagued by two factions, just like people anywhere else.

BRIAN PULLAN
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WALTER PREVENIER and WIM BLOCKMANS. *The Burgundian Netherlands*. Assisted by AN BLOCKMANS-DELVA. Foreword by RICHARD VAUGHAN. New York: Cambridge University Press. 1986. Pp. 403.

This impressive book surveys the development of the Netherlands into an economic, social, political, and intellectual community between 1380 and 1530. Whereas Philip the Bold, the first Burgundian count of Flanders, was resented as a foreigner, his great-great granddaughter Mary (died 1482) was revered as a bulwark against the French. Walter Prevenier and Wim Blockmans contend that, whatever the original intentions of the Burgundian rulers may have been, they hoped to form a state independent of both France and the Empire by 1428 at the latest, when their succession in Holland and Zeeland was guaranteed. Thenceforth, the dukes were rarely in Burgundy, spending virtually all their time in the Netherlands. Yet each principality of this composite state kept its separate governing institutions, and until the time of Charles the Bold few administrative measures were undertaken for the entire Netherlands.

The most original and controversial sections are the first five chapters. The authors divide the Netherlands into zones of urbanization with a distinct rank-size pattern and develop a thesis of urban regionalism. They find a high incidence of poverty in both the urban and rural areas but base their calculations on a juxtaposition of sources that are widely divergent in time and genre. Their best sources are tax records, but it is egregiously misleading to equate "tax poverty" with indigence. Indeed, the authors admit that the rural tax figures are distorted downward by the fact that many wealthy landowners bought citizenship in the towns as a tax shelter for their rural properties and that, whereas 79 percent of the inhabitants of a workers' quarter in Ypres in 1431 were too poor to pay a tax, only 13 percent of them were poor enough to obtain assistance from charitable foundations. The authors are

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on firmer ground in arguing that the Burgundian Netherlands was much less severely affected by the general economic depression of the late Middle Ages than were most other regions. The volume of internal trade nearly doubled between 1400 and 1480, foreign trade prospered, and town economies became more diversified and broke away from their dependence on single exports or services. Basing their constructions on tax levels, wage figures, and grain prices, the authors conclude that, after a period of recovery from the civil war that ended in 1385, the years 1400–08, 1440–75, and 1492–1530 were prosperous, whereas the intervening years were hard. But these conclusions show their bias in favor of the cities, for they define as prosperous periods when taxes were low, wages were high, and food prices were depressed.

The chapters on the arts are useful summaries that contain few surprises but emphasize the social and economic context. Netherlandish culture broke away from French influence and established its own originality and vitality in the fifteenth century. The aristocratic marriages made by musicians and painters show that they enjoyed high status. Patronage was centered on the dukes' "theater state," and the rulers consciously fostered the arts as a means of influencing public opinion and a Burgundian consciousness that transcended local particularism. But local governments and many private citizens were also patrons of the arts, and there was a growing market for art to adorn private homes.

Although intended as a synthesis and with an unfortunately abbreviated bibliography, the book summarizes a vast amount of original research on social and economic structures and governing institutions, much of it by the authors themselves. Readers unfamiliar with Dutch must use this study as a starting point for any examination of the subject, for the recent and still accessible works of Richard Vaughan are political biographies of the dukes, and all surveys of economy, society, and statecraft that might be compared to this are hopelessly outdated. The lavish illustrations are valuable in themselves but unfortunately drive the price out of the range of most potential readers.

DAVID NICHOLAS
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JONATHAN RILEY-SMITH. *The First Crusade and the Idea of Crusading*. Philadelphia: University of Pennsylvania Press. 1986. Pp. 227.

Jonathan Riley-Smith's analysis of the formation of Crusading ideology offers a provocative new interpretation of the process. Riley-Smith maintains that the doctrine that underlay Urban II's proclamation of the First Crusade was in the main fairly conventional. Urban's appeal differed from earlier efforts to arouse interest in war against Islam, however, by focusing on Jerusalem as the goal of the expedition. Urban directed his message primarily to French-speaking peoples, and that, Riley-Smith argues, also accounted for much of the enthusiasm that it elicited. The timing of Urban II's plan further enhanced its success. By 1095 the belief that active intervention in the Mediterranean was needed to push back Islamic advances was widely shared by the public. These three factors, according to Riley-Smith, account in the main for the astonishing (and perhaps unanticipated) success of the Crusading message.

Riley-Smith argues that, once the Crusade was under way, both the ordeals that the participants experienced on their journey and the spectacular success they achieved at its close induced a conviction of righteousness and a euphoria that shaped later Crusades—none of which enjoyed anything approaching the same gratifying outcome. During the generation that followed 1099, the argument continues, Western writers transformed the First Crusade into a legend, and theologically minded chroniclers, such as Robert the Monk, Guibert of Nogent, and Baldric of Bourgueil, pictured the Crusade as a vast "military monastery on the move" (p. 2). In the process they created an ideological underpinning for further Crusading ventures. The result was a Crusading ideal that continued to fascinate Europeans for generations, even after the reversals that culminated in the fall of Acre and the expulsion of the Latins from the mainland of the Levant. This in brief is Riley-Smith's argument.

Riley-Smith's scholarship is impeccable, and he supports his contentions with abundant citations from contemporary documents. He could, I think, have carried his line of argument further in at least one respect. Not only did Urban II's Crusading message require further refinement before it could achieve theological maturity, as Riley-Smith suggests, but in addition the discipline of theology itself needed to develop further than it had in 1095 before it would become possible to elaborate a satisfactory Crusading theory. This was clearly true, for example, with the indulgence. The difficulty that the Crusades raised was at bottom the problem of integrating a doctrine of holy violence into the larger body of Christian thought, and Christian thinkers still wrestle with this problem. Riley-Smith's book is a distinguished and persuasive contribution to the history of the problem's development.

JAMES A. BRUNDAGE
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NORMAN P. ZACOUR and HARRY W. HAZARD, editors. *A History of the Crusades*. Volume 5, *The Impact of the Crusades on the Near East*. Madison: University of Wisconsin Press. 1985. Pp. xxii, 599. $40.00.

This fifth volume in the monumental *History of the Crusades* series focuses on the Crusader states of the Near East as Western colonial settlements. Topical chapters describe related subjects such as reasons for the Latin conquest; Arab cultural life and Latin influence on conquered Muslim lands; political, economic, and religious organization of the conquered territories, including Latin-native relations; demographic factors; the Teutonic Knights, illustrative of a military order generated by the Crusading movement; the colonial activities of the Venetians; and the Franciscans' and Dominicans' Near and Far Eastern missionary activities.

The colonial aspects of the Crusades may well be the most important area of scholarly writing about the movement in recent times, and the present work makes available the results of a great deal of it in a single volume. Another asset is having as contributors such distinguished scholars as Jean Richard and Joshua Prawer, who have contributed so much to the reputation of Crusading colonialism as a subject. The work also benefits from the same scholarly apparatus of earlier volumes: extensive bibliographies of each chapter's subject, footnotes, maps, and gazetteer. The lack of subject indexing, however, detracts a bit from its value as a reference work.

Some material that logically belongs to the thematic emphasis of this work appears elsewhere in this series, most notably the subjects of colonial art and architecture discussed in volume 4. However, several subjects that have received some attention in earlier volumes and are treated in passing in this one do not receive the comprehensive attention they deserve in virtue of their colonial significance. Among the slighted subjects are Outremer's major military orders, the Templars and Hospitallers, and, to some degree, Genoa. Omission of an in-depth study of the maritime republic comparable to Louise Robbert's excellent essay on Venice resulted from the death of its intended author, Vsevolod Slessarev. Robbert remedied the situation somewhat by including references to Genoa in her chapter.

Editors Norman P. Zacour and Harry W. Hazard have taken enormous care in compiling this volume so that simple factual slips are very few. However, a number of points are open to challenge. One is the deterministic tone of the conclusion of the chapter on population. Although demographic factors certainly influenced aspects of the Crusading movement and Crusader civilization, I am more comfortable with explanations for the failure of the Crusades such as the Latins' political and military mistakes and their various rivalries than "demographic realities, notably in Anatolia and Egypt" (p. 314). Another is Richard's belief (influenced by La Monte) that King Amaury I (1163–74) is the likely founder of the Latin Kingdom of Jerusalem's maritime court and port toll collection bureau, the Court of the Chain or *catena* (p. 224). Richard bases his position, apparently because of a perceived absence of earlier evidence, on King Amaury's reputed authorship of a collection of maritime laws. However, charter evidence supports the existence of the *catena*, like its inland market counterpart, the *funda*, before the reign of Amaury I, possibly as early as King Baldwin II (1118–31) (see *Speculum* 39 [1964]: 474–77). The laws attributed to Amaury I (assuming that the king is Amaury I and not Amaury II) are evidence of his contributions to maritime law; these *assises* do not establish that the *catena* began functioning as a court with Amaury. Several passages that mention the *cour de la chaine* can be interpreted as referring to an already existing institution. There also is evidence that a maritime law banning trade in militarily valuable goods with Egypt, found among Amaury's *assises*, already existed in some form under Amaury I's predecessor, Baldwin III (1143–63) around 1156 (see Reinhold Röhricht's *Regesta Regni Hierosolymitani* [1893–1901], vol.1, no. 322).

This work's enormous value deserves stressing. Readers will gain a greater appreciation not only of Latin Outremer and of medieval colonialism but also of some Western institutions that can be viewed in their adaptation to a new and often alien environment. Volume 5 takes its place as a distinguished member of its series, as an impressive monograph in its own right, and as a reference tool scholars will value.

ROBERT B. PATTERSON
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LESLIE J. MACFARLANE. *William Elphinstone and the Kingdom of Scotland, 1431–1514: The Struggle for Order*. Aberdeen: Aberdeen University Press. 1985. Pp. xix, 517. £30.00.

The world remembers Bishop Elphinstone of Aberdeen, insofar as it remembers him at all, as the founder of the University of Aberdeen. That was a remarkable feat, since it gave a poor, economically backward, thinly populated country a third university at a time when its rich southern neighbor had but two. Leslie J. Macfarlane rightly regards this as the climax of Elphinstone's long and distinguished career. He devotes over one hundred pages of this massive study to a careful and loving account of Elphinstone's achievement, from the high politics of obtaining royal and papal support for his plan down to the details of the curriculum, a who's-who of the

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faculty, and a diagram of the seating arrangements in the choir of King's College chapel in 1514. This short monograph is the most original section of the book and, by itself, would justify the writing of this volume.

But there is much more to Macfarlane's work than that. He is not much concerned with Elphinstone's personality, which, indeed, does not come through very clearly. Elphinstone had a long and successful career in both church and state—he died at eighty-three—and Macfarlane uses the stages of that career as a kind of clothesline on which to hang a series of essays on various aspects of the fifteenth century. So we get accounts of the nature of higher education in France, where Elphinstone studied, the operations of the canon law and ecclesiastical courts, and an excellent chapter on Scottish foreign policy, in which Elphinstone was much involved in the last decade of the reign of James III.

Above all, however, Macfarlane is interested in the problems of governance in both church and state in Elphinstone's lifetime. Elphinstone was a careerist; he obtained his bishopric because he was an able royal servant whom James III wished to reward. Unlike many other careerists, he paid close personal attention to some aspects of diocesan administration. He successfully asserted his rights of appointment and increased the bishopric's income. The money was spent in Aberdeen itself, on the university and on the cathedral, whose fabric he tended and where he provided vicars choral to embellish the services. He did little, if anything, for the parishes of the diocese, all but four of which were appropriated. Macfarlane argues that the educational level of the parish clergy improved in Elphinstone's time, although the evidence on this point is not altogether clear. Macfarlane is charitable toward such abuses in the church as nepotism, pluralism, and even the large number of clerical bastards, of whom Elphinstone himself was one. He even characterizes Sixtus IV as a "reforming pope" (p. 257) and uses Elphinstone's diocesan achievements to argue that "the situation [of the church] was still remediable, and the temptation to see it as irreformable has to be resisted" (p. 278). This argument is unconvincing. Given the crown's powers of appointment, consistent royal backing was essential for reform. Yet James IV, whose support was vital to the creation of the University of Aberdeen, used the archbishopric of St. Andrews as a sinecure for his younger brother and then for his eleven-year-old son. "As far as the spiritual interests of the Scottish Church was [*sic*] concerned," Macfarlane admits, this was a "disaster" (p. 313). Macfarlane does not think well of James IV, whose extravagance and fatuous foreign policy he contrasts unfavorably with the fiscal and foreign policies of his father. The account of James III is decidedly revisionist. His outlook and policies were much like those of Henry VII, but he did not provide leadership, he was insufficiently ruthless, and he did not have the personnel available to prevent a crucial breakdown in the judicial system in the face of a flood of litigation. Once again, the argument is not entirely persuasive. But the fact that some of Macfarlane's interpretations are open to question in no way diminishes the impressiveness of his scholarly achievement. This is a splendid book, which should be read by anyone interested in the political, ecclesiastical, or intellectual history of late medieval Europe.

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MODERN EUROPE

JONATHAN I. ISRAEL. *European Jewry in the Age of Mercantilism, 1550–1750.* New York: Clarendon Press of Oxford University Press. 1985. Pp. xii, 293. $34.50.

Jonathan I. Israel's book is a well-written, refreshing, stimulating work that presents as an integrated whole a lengthy period in Jewish history that has all too often been viewed merely as an erratic and painful transition from the late Middle Ages to modernity. Israel argues that between the midsixteenth and early eighteenth centuries the Jewish diaspora, extending from the New World to Eastern Europe and the Ottoman empire, constituted a cohesive social, economic, and cultural network unlike that of any other period in Jewish history. In support he assembles an impressive and variegated array of materials on the Jewish population of every part of Europe, from the "crypto-Jews" of the Iberian peninsula and the Sephardic communities that they established in the Mediterranean, Amsterdam, southern France, Hamburg, London, and the Caribbean to the mixed Jewish population of northern and central Italy and the Ashkenazi settlements in Germany and Austria, Bohemia and Moravia, and Poland-Lithuania.

The rise and fall of this era of Jewish enterprise and creativity he discerns as follows. The years between 1470 and 1570 saw a near destruction of Jewish life and learning in Western and Central Europe, beginning with large-scale expulsions from Spain and Portugal and continuing with local Central European expulsions in which anti-Jewish sentiment was reinforced by Lutheran and humanist attitudes and the Catholic Counter Reformation. Between 1570 and 1600, however, a wave of settlement occurred in areas that had previously excluded Jews or had been inhospitable to them. The

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new Jewish communities of the Atlantic ports and in Germany were consolidated during the first two decades of the seventeenth century. During the Thirty Years' War some of their members were in a position to provide valuable services to European rulers. These benefits were paid for not with hard cash but with concessions and privileges of which the Jews alone had the need and which, given growing *politique* and mercantilist attitudes, the rulers were now quite willing to concede, despite local and clerical opposition.

Israel's analysis dwells in greatest detail and with special effectiveness on the years from 1650 to 1713. At that time Amsterdam was a pivot of Jewish economic activity, and the Central European court Jews, a scattered elite of provisioners and financiers who were also leaders in their Jewish communities, were at the height of their influence. These decades seem to have been marked by an upsurge of Jewish population almost everywhere (only temporarily interrupted by the massacres of the Jews of the Ukraine in 1648–49) and by the strengthening of the interlocking system of autonomous Jewish judicial and welfare institutions that maintained communal vitality and self-discipline.

Israel adroitly lays out the elements of the Jewish literary and religious revival of the sixteenth and first half of the seventeenth centuries. This upsurge culminated in the spiritual turmoil of the later seventeenth century, with its mix of antinomian Jewish messianism, a "Spinozistic revolt" among a few Amsterdam Sephardic intellectuals, a self-assertive burst of synagogue construction, and a Jewish apologetics that played a role (so Israel argues) in the undermining of traditional Christian belief. Israel concludes with a brief account of the weakening of Jewish communal and religious life in the eighteenth century. The defensive, almost ideological, slant in his remarks on the continued process of Jewish absorption into the mainstream of European civilization does not detract from the value of his synthesis as an important delineation of the early modern era as a novel and unified phase in Jewish history.

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OTTO MAYR. *Authority, Liberty, and Automatic Machinery in Early Modern Europe*. (Johns Hopkins Studies in the History of Technology.) Baltimore: Johns Hopkins University Press. 1986. Pp. xviii, 265. $30.00.

Otto Mayr's original and delightful book instantiates—the author prefers to say proves—the proposition summarized in its concluding sentence: "Technology . . . is part of the intellectual life of a culture" (p. 199). Stated so baldly, the point is no more than what has been asserted as a matter of course by most if not all writers on technology and its history since late in the eighteenth century. Whatever the ideologies served, and they have been manifold, the besetting weakness of that literature has ever been its failure to exhibit concrete and convincing relations between its sociological and technical components. Mayr's analysis cuts closer to connective tissue than any other known to me.

It does so by noticing and then exploring the contrast in fortune between two types of machinery in two social contexts. The mechanisms are clockwork and feedback. The societies are Continental Europe and Britain from the late Middle Ages through the eighteenth century. Mayr came to the problem from an investigation of why feedback, which was understood and employed in antiquity, was ignored and even rejected by machinists and engineers of the Renaissance. Why, further, did the principle come into its own only in Britain in the Industrial Revolution?

The problem is less the facts than the factors, and the author approaches them by turning first to the study of a positive control, the success story of the mechanical clock following its appearance near the turn of the fourteenth century. Mayr is excellent on the technical history of the clock in regard to the crucial importance of the escapement and to the three phases of weight-driven, spring-driven, and pendulum-regulated timepieces. His iconography is fine, and the illustrations dramatically bring out the theme of a baroque drive for mastery over nature in European culture. But he is at his best in the literary reaches of his subject. The metaphor of the clock as the symbol of order and regularity, political and cosmic, testifies to its technological centrality in the European social system based on the principle of authority. In England, on the other hand, authority was challenged in the name of liberty, and, from Elizabethan times onward, literary allusions to clockwork tended toward the grudging, treating its presence as at best a necessary evil and at worst an unwelcome reminder of the tyranny of time and other agencies.

Mayr leaves himself less than one-third of his admirably brief book to treat the topic that motivated it, the symbiosis between self-regulating mechanisms and the emergent liberal social order in eighteenth-century Britain. The asymmetry may be appropriate. As the author says, one feature of feedback is that it is unobtrusive when working well. Unlike a showy timepiece, it does not appear to be doing anything. Moreover, the themes of equilibrium in the political order, checks and balances in government, and the feedback loop in Adam Smith and in the free market are all relatively familiar.

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A few reservations suggest themselves. The diametric contrast between an authoritarian Europe and a libertarian Britain is more consonant with the historiography of a generation or two ago (not that it is necessarily wrong for that) than it is with contemporary scholarship in social and economic, or even political and constitutional, history. Other questions attach to the genre, which is *Kulturgeschichte*. Mayr is equally conversant with American and German scholarship. He wears lightly the erudition that he derives from the latter. Still, abstractions serve freely as historical actors, and many concepts such as the Renaissance and society are invested with will and purpose and figure grammatically as the subjects of transitive verbs. This is simply to say—and I mean it far more as compliment than criticism—that his persuasive and beautifully written book is reminiscent of Max Weber and *The Protestant Ethic and the Spirit of Capitalism*.

CHARLES C. GILLISPIE
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JAMES W. MCCLELLAN III. *Science Reorganized: Scientific Societies in the Eighteenth Century*. New York: Columbia University Press. 1985. Pp. xxix, 413. $45.00.

In this study of the proliferation of official state scientific societies and academies throughout Europe in the eighteenth century, James W. McClellan III seeks to make clear that science during the Enlightenment need not be relegated to Joseph Ben-David's "scientistic" interregnum nor abandoned to the uncaring arms of the dilettante. Relying on nearly a dozen archival sources as well as published memoirs and a large number of studies of individual organizations, McClellan chronicles the inception of the major official, semiprivate, and private institutions, including those that were concerned with the mechanical arts, the economy, agriculture, and medicine. He delineates their structures and their elite memberships and tells us about their encouragement of research and their growing interest in common ventures to test, for example, the Newtonian and Cartesian theories of the earth's shape or to observe the transits of Venus.

Although the mass of information presented in McClellan's erudite study constitutes an important contribution, the book unfortunately often reads like a directory. What stands out boldly, in contrast, is the luster of some of the individual savants—a useful term, I believe, because it was self-descriptive and well describes the concerns of men such as Jean d'Alembert but especially of individuals with highly diverse interests, such as Montesquieu. Their nonspecialized interests, as well as, in some cases, their expert knowledge, helps explain the transitional state of science in the eighteenth century.

Unfortunately, McClellan makes too little of these unique features of the eighteenth century. By trying to see the influence of the social and intellectual settings of the period on the learned world's activities, he is in good company, but the distribution of the weight between context and intellectual content might have been more judiciously balanced. By putting too much stress on context, with institutionalization as its primary subset, McClellan gives preference to activities that were early established as scientific. He is also persuaded that the mapping of institutions will explain the development of science, but he forgets that their mixed memberships militated against uniform signs of scientific productivity. Fruitful scientific activities, such as astronomy, mathematics, rational mechanics, and electricity, were informed by patterns of thought and practices that may be assimilated to the firmer epistemologies that elaborated the legitimacy and professionalism of science in the nineteenth century. Thus, even though McClellan wants to exclude the problem of scientific specialization, his treatment implies that advances in specialized areas were the decisive determinants of the socialization of science within the precincts of the societies and academies he has scanned. In the process he ignores the status of natural philosophy and natural history, whose scope was more sweeping and less exclusive, and he averts his eyes from the "public show" aspects of the organizations. The publicity surrounding the exclusion of Franz Mesmer and Jean Paul Marat from the ranks of the *cognoscenti*, for example, need not be interpreted as proof of the power of an uncontested cognitive authority over persons who did not hesitate to gain some of the public favor that the learned men of the *Académie* tried to monopolize. A more incisive theoretical structure is needed to reveal and account for the various kinds of ideas and practices to which these umbrella organizations accorded uneven degrees of status and prestige. They were doubtless instruments of power, and Samuel Formey's depiction of them in the late 1760s as capitals of the sciences reinforced the self-esteem the savants needed.

McClellan declares that his aim is the study of the eighteenth century on its own, yet he does not fully confront some of the more subtle questions of a culture whose intellectual tensions were heightened by diverse interests and practices, not the least of which was the role played by the state in sponsoring scientific societies.

HARVEY MITCHELL
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SIÂN REYNOLDS, editor. *Women, State, and Revolution: Essays on Power and Gender in Europe since 1789*.

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Amherst: University of Massachusetts Press. 1986. Pp. xvi, 190.

The eight essays in this collection originated as a set of lectures given at the University of Sussex. Although Siân Reynolds, in her introductory remarks, correctly highlights the strikingly unusual historic presence of European women in revolutionary situations, the volume addresses that topic to a surprisingly limited degree. It is more accurate to see the book's focus as women's relationship to public or state power. All examples are drawn exclusively from the modern European context. The essays by theme may be grouped into three broad categories: two are predominantly biographical, four treat the historical experiences of women in political though nonrevolutionary situations, and two (also the most original and important) deal with the iconographic presence, imagery, and media representation of political women.

Mary Wollstonecraft and Alexandra Kollontai are singled out for biographical analysis. The reasons for so doing should have been made explicit. Irene Coltman Brown's essay on Wollstonecraft is based on a close reading of her major and minor writings and provides a good summary of her life and ideas, whereas Beryl Williams's piece on Kollontai retells the by now familiar story of the visionary theorist who dared pose radical solutions to the challenges faced by "new" women in a revolutionary society. Neither effort is strikingly novel or insightful.

In sharp contrast are the remaining articles in the collection. In her essay on nineteenth-century France, Michelle Perrot discusses the dominant ideas of the period on the question of women and power. Seeking to overturn conventional historical perspectives, she reminds us of the reality of women's presence in the most everyday forms of life, showing how the activity of nineteenth-century French women consisted above all in organizing the private, familial, and maternal power assigned to them. Reynolds looks at the way political power was constructed without women in France after 1789. In a provocative discussion she unmasks the "maleness" of French republicanism in its vocabulary and points of reference, calling into question republican celebration of the achievement of universal suffrage. Despite the persistence of "unisexual" suffrage into the twentieth century, she explains how republican historiography has had comparatively little to say about how and why women got the vote; its focus, rather, has been why they did not get it, the history of the perceived dangers female suffrage would pose to the republic. In addition she emphasizes the paradoxical fact that real women were excluded from a republic whose imagery and iconography were so overwhelmingly female.

In presenting the curious disparity between Marianne as the symbolic representation of republican France and women's actual place within the republic, Reynolds stresses a recurrent problem noted by other essayists: women's symbolic or representational function in relation to public power. The most stimulating essays in the collection have as their subject the visual arts and the contemporary media, addressing the dual and competing images of the political woman as inspirational symbol and dangerous harpy. In any study of women and politics, especially revolutionary politics, Eugène Delacroix's figure of Liberty—often wrested from her context—looms large as powerful testimony to the abiding power of women as symbols in revolutionary politics. Marcia Pointon's thesis is that art historians have wrongly assumed Madame Liberty to be an allegory or a figure representing the proletariat without noticing the sexual power at the painting's heart. The myth of the violent, sexually neurotic woman driven into politics by her irrationalism is best seen, according to Ulrike Hanna Meinhof, in contemporary media representations of two groups, the West German women of the Baader-Meinhof group and the peace activists of Greenham Common. Using accounts in sections of the British popular press, she shows how the paradoxical description of the Greenham women as sexually deviant and the reinterpretation of their political protest in sexual terms is an obvious instance of the convergence of the sexual and political.

Two essays on twentieth-century themes round out the collection. Sybil Oldfield makes a useful contribution in her study of female resistance to Hitler, tracing the lives and activities of numerous women who have remained almost completely unknown outside the two Germanies. Anne Stevens discusses the hostile environment and personal hazards faced by aspiring female politicians and civil servants in contemporary Britain, France, and Germany.

Although this volume is uneven in terms of its overall contribution to the historiography of women's public protest activity, its importance lies in making available to English-language readers the work of several leading women's historians from continental Europe whose writings are not yet widely available in translation. Unfortunately, much of the prose is dense and difficult, thereby limiting the book's value as an undergraduate text.

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LAWRENCE STONE and JEANNE C. FAWTIER STONE. *An Open Elite? England, 1540–1880*. New York: Clarendon Press of Oxford University Press. 1984. Pp. xxv, 566. $39.95.

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Lawrence Stone and Jeanne C. Fawtier Stone have a question in their title but none in their conclusion. They test a familiar thesis—in England there was "frequent and easy upward mobility of successful men of business into the ranks of the landed elite" (p. 4)—over 340 years and declare it false. The infiltration of the upper landed elite by "base-born *nouveaux riches*," it is said, was always small, the "contrast in reality" with other countries far less than previously supposed. All that remains, the Stones conclude, is to explain why the myth of an open elite enjoyed such extraordinary longevity and ubiquity, accepted as it was from the Reformation to the twentieth century by English and foreigners alike.

Few will question the Stones' general thesis. Over the past twenty years, as J. V. Beckett has shown in *The Aristocracy in England, 1660–1914* (1986), specialists have edged toward the same conclusion: the relatively closed nature of the English landed elite. But the Stones do more than place an authoritative imprimatur on recent findings. They use an ingenious method, they examine the membership of the English aristocracy over three and a half centuries, and they amass a wealth of useful information about the landed elite.

In their studies of upward mobility, most scholars have counted the number of new men of wealth among the English landed elite, but the Stones, taking another approach, make the owners of large country houses the subjects of their study. These, the "country gentry," are distinguished from the "parish gentry" in the land by having at least one house "of a certain minimum size, standing in pleasure-grounds of a certain minimum acreage" (p. 11). A house of minimum size for the great gentry is defined as one with at least fifty units of living quarters with each unit consisting of 100 square feet of floor space. The bulk of the study is devoted to a careful analysis and quantification of the owners of these "mausoleums of vanity," but in some one hundred pages the large houses themselves are examined as symbols of elite status. The section on houses ("Social Functions," "Cultural Ideas and Financial Constraints," and "Buildings and Builders") adds much to the value and the color of the study, but it could stand as an independent essay.

The data for the book are drawn from three English counties of diverse economic and social experience. Northamptonshire was predominantly rural; Hertfordshire, one of the home counties, was only a dozen miles from London; and Northumberland, with "the best modern multi-volume survey in the whole of England" (p. 47), was a northern county with a large coal industry and the city of Newcastle. The Stones give well-considered reasons for the selection of these counties, and scholars will probably find that the three are representative of the whole of the British experience. No reason is offered for the exclusion of Scotland, Wales, and Ireland.

The selected counties yield a sizeable sample—2,246 owners of 362 houses—and a mass of information, "up to 160 pieces" about each owner under review. The statistical evidence, arranged in 53 graphs in the text and in 64 tabular summaries at the end of the book, covers such subjects as county endogamy, inheritance, surname changes, house sales, purchasers' social origins, housebuilders, and building activity.

The Stones conclude that only 137 men of business, or 6 percent of all owners, bought their way into the "country gentry" of Northamptonshire, Hertfordshire, and Northumberland. There was, therefore, a "relatively durable and extremely tenacious core" (p. 403) of older families, and this stability ensured the continued social and cultural preeminence of the landed elite from 1540 to 1880. The Stones describe how, in changing circumstances, families managed to preserve a link between a house, an estate, and a family name. By ingenious means the "country gentry" overcame even the greatest threat to the stability of their group: a biological decline, especially acute in the late seventeenth and early eighteenth century, when many great families failed to reproduce themselves.

Specialists will soon test the authors' conclusions in other English counties and in other parts of the British Isles, but all future studies of the landed elite will be measured, in turn, against this powerful and persuasive study. This debate over the composition of the English aristocracy is of no small importance because a number of comprehensive interpretations of English history rest on the truth of the paradigm of "an open elite."

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BARBARA J. HARRIS. *Edward Stafford: Third Duke of Buckingham, 1478–1521*. Stanford: Stanford University Press. 1986. Pp. viii, 334. $37.50.

Edward Stafford, duke of Buckingham, was the only peer of his rank in 1485 and the largest private landowner in the country, but his political achievements failed to place him in the front rank of the Tudor nobility. For all his wealth, royal pedigree, and style, Buckingham is remembered chiefly because he was tried and executed for treason in 1521 at the age of forty-three. Barbara J. Harris has developed an interesting organizational approach to her topic by creating a series of concentric circles that move outward from the duke to his family and estates and finally to his relations with the crown. Buckingham's family tree is a fascinating microcosm of the late medieval nobility and includes ties with

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the Beauforts, Percies, Nevilles, and Howards. Despite his impressive lineage, Buckingham behaved ignobly and arbitrarily toward his tenants, whether gentry or serfs (whose numbers increased on his Welsh estates). His repudiation of traditional concepts of good lordship was eloquently expressed when he urged his officers to "drive" the tenants "as much to our profit as you can." On the other hand, the author argues quite convincingly that Henry VII treated the young duke so badly that disenchantment with the Tudor monarchy was almost inevitable. She also shows that Buckingham was never able to exercise significant political power. He rarely attended the king's council and was not sufficiently trusted to be given major military commands. He amused himself by spending a fortune on gorgeous apparel and enjoyed nothing more than exhibiting himself at all manner of ceremonies, feasts, and pageants. In a closely argued final chapter, Harris concludes that Buckingham received a fair trial by the standards of the age and that his claim to the throne threatened Henry VIII and caused his execution.

Harris is the second scholar to have studied the third duke of Buckingham in recent years. In *The Staffords: Earls of Stafford and Dukes of Buckingham, 1394–1521* (1978), Carole Rawcliffe examined the duke's landed estates, life style, and unsuccessful litigation in the Courts of the King's Bench and Common Pleas and concluded that he was a victim of changing times. Although Harris is less sympathetic to Buckingham, she has not attempted to repudiate the major arguments of Rawcliffe's work. The two authors differ on a few issues, namely, the cost of the duke's retinue, the membership of his household, and his role in the Renaissance, but Harris places greater emphasis on early Tudor politics and the treason trial. Taken together the two books probably tell as much about Buckingham as can be learned from surviving sources. Harris's volume is carefully researched, well organized, and readable. By focusing on the greatest of the older Tudor nobles, the book helps correct the notion that the new nobility of the mid-Tudor period were grasping upstarts who behaved altogether differently from their more worthy predecessors of ancient lineage. In truth, the new nobility with all their warts and vices were the legitimate heirs of Edward Stafford, duke of Buckingham.

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HELEN MILLER. *Henry VIII and the English Nobility*. New York: Basil Blackwell. 1986. Pp. 286. $45.00.

Helen Miller aims to demonstrate the formal relationships between the nobility and the monarch. She does not set out to examine the role of the nobility in society. The clients and affinities of the noble class figure hardly at all.

Within the limits selected Miller proceeds in a very orderly and efficient manner using a thematic approach. The first third of the book is about creation of peers, promotion of peers, and the elimination of noble traitors. There are few surprises. By making a modest number of creations, Henry added to the glory of his court while preserving the nobility's status. The holding of a major office and successful military leadership were the most common reasons for promotion. Miller does not suggest that a desire to balance the strength of factions played a part. Treason trials (before the high steward) and attainder acts were a way of disciplining the nobility through exemplary punishment. The nobles made to participate in convicting their errant fellows in court or through Parliament were compliant in every case save one.

The middle chapters of the book are concerned with attendance at court and service on the council, in Parliament, and in war. Apparently, Henry made no particular use of the court as an instrument of observation and control, and the time individual nobles spent there was limited. The nobles played only a small role in the work of council, largely because of the dominating presence of Wolsey and his emphasis on its judicial functions. The nobility also did not achieve any greater participation in the council's activities after the reorganization in the later part of the reign. The lords attended Parliament not to play politics but rather to pursue and protect family interests. Nevertheless, it was in Parliament that they had the best opportunity to advise the crown. In contrast their role as commanders and contingent bringers in time of war was important and the participation rate in campaigns was high. Successful performance in the field might make a nobleman an intimate of the king for life.

The last third of the book, entitled "The King's Rewards," deals with grants of office and lands. Supplication (and sometimes a gift in addition) was necessary for both. Most successful in this search were the nobles closest to Henry or ennobled by him. Those of the higher nobility were usually able to secure the granting of a local office of some sort, but over half of the nobles never so benefited. The king was no more generous with gifts of land, and the chief beneficiaries were the same men. The overall picture is therefore of a nobility well under the king's control and rewarded sparingly because there was no need to buy its favor.

Substantial holding of land was surely the essence of noble status, yet Miller eschews suitable comment on the changes to the land law Henry's government imposed, changes that did much to hasten the world of bastard feudalism to its end. This is unfortunate.

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The book will nevertheless be a useful work of reference.

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G. W. BERNARD. *War, Taxation, and Rebellion in Early Tudor England: Henry VIII, Wolsey, and the Amicable Grant of 1525*. New York: St. Martin's or Harvester, Sussex. 1986. Pp. xi, 164. $32.50.

G. W. Bernard is rapidly establishing himself as one of the most active members of the younger generation of Tudor historians. In 1985 he published his first book, *The Power of the Early Tudor Nobility*, a study of the earls of Shrewsbury. The present volumes follows hard on its heels.

The subject, though a small one, is particularly interesting. In 1525 Henry VIII was convinced that he had an unusual opportunity to recover French territory lost in the Hundred Years' War and possibly to gain the French crown itself. He had recently been involved in an unsuccessful French campaign, which had been financed by a large parliamentary grant of taxation and by substantial loans that were never repaid. Grasping for a new source of revenue, the king and his chief minister, Cardinal Wolsey, determined to ask subjects for an "Amicable Grant" of money, not voted by Parliament and not accompanied by promises of repayment. Opposition developed almost immediately, there was one small-scale insurrection, and the king ultimately abandoned the entire scheme. Not a penny was paid, and no English forces crossed the Channel.

Bernard is concerned to determine why the king's plan misfired. He discounts the suggestion, presented by earlier historians, that Wolsey secretly opposed the operation and worked behind the scenes to discredit it: Wolsey and Henry, Bernard concludes, had consulted fully and shared the same goals. If Wolsey preached pacifism, that was mere rhetoric and not a sign of genuine aversion to military activity. Bernard also refutes charges that the dukes of Norfolk and Suffolk, who were responsible for collecting the grant in the counties where the greatest opposition surfaced, were disloyal or ineffectual, and he exonerates William Warham, the archbishop of Canterbury, from allegations that he encouraged resistance in Kent because of his own opposition to Wolsey. In fact, Bernard argues, the two archbishops enjoyed cordial relations. In a final revisionist chapter Bernard dismisses the suggestion, originally made by the chronicler Edward Hall, that people refused to pay because of concern that the government was acting unconstitutionally in demanding money without consulting Parliament. There is no contemporary evidence for such a motive.

It was simply poverty, Bernard believes, that caused the failure of the grant. Several causes lay behind this: the final installments of the tax granted in 1523 were still being collected; there was a shortage of coin, possibly resulting from the export of fine English coinage to the Continent; the textile industry was particularly depressed, a special situation that lay behind the uprisings in and near Lavenham, one of the country's richest clothing towns.

In refuting traditional opinion, Bernard uses the scanty surviving evidence with realism and common sense. He frequently puts forward alternative interpretations and shows why they are unlikely to be correct. He does not enter into a substantial discussion of Wolsey's foreign policy, so one is not entirely convinced by his arguments that the cardinal supported the king's militaristic position. In other areas his revisions are compelling and will have to be taken into account when the history of war and taxation in Tudor England is rewritten.

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MARIA DOWLING. *Humanism in the Age of Henry VIII*. Wolfeboro, N.H.: Croom Helm. 1986. Pp. 283. $43.00.

ALISTAIR FOX and JOHN GUY. *Reassessing the Henrician Age; Humanism, Politics, and Reform, 1500–1550*. New York: Basil Blackwell. 1986. Pp. vi, 242. $45.00.

The history of humanism remains an awkward subject. In England particularly both the meaning of the term and the historical significance of the movement are still in doubt. The insularity of English scholarship has not helped, since, whatever one may make of the matter, it was clearly a thoroughly international affair and the English episode only a belated and provincial response. Even the chronology of the subject remains dubious since humanism has been discovered at various times in the Middle Ages, in the fifteenth century, in Renaissance Europe, and throughout the early modern period down to the time of the English Augustans. Both books under review resolutely fasten on the reign of Henry VIII, scarcely recognizing the chronological and geographical difficulties, and, although they notice some of the interpretative problems in the subject, neither, I fear, addresses them very satisfactorily.

Maria Dowling has turned a doctoral dissertation that she called *Scholarship and Politics at the Court of Henry VIII* (1981) into a book. I have not seen the original, but the old title would do as well or better than the present one. The author appears to be more concerned with the sponsorship and patronage of a miscellaneous set of literary activities by the

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court and its followers than with the sum and substance of the new learning. She defines humanism broadly, "as the reappraisal of religious and secular thinking through examination of the literary bases of theology and philosophy: that is, the Christian scriptural and patristic writings and the 'acceptable' pagan classics" (p. 1). This, she claims, entailed a knowledge of the Hebrew, Greek, and Latin sources (although hardly anyone attained to all three), a concern for linguistic accuracy and the purification of Latin (although English philology was only just beginning), some translation (a defeat, surely, for the classicists who preferred the originals), and an admiration for classical style (though again more aspiration than reality). Apparently, anyone involved in any of these things, from Erasmus to Anne Boleyn, who are given about equal treatment, is qualified for inclusion.

Just how or whether these activities fit together or why indeed they should have appealed to the English in the reign of Henry VIII is left obscure. We learn little about how they began, what propelled them, or where they led. Instead, we are warned that humanists generally disagreed about most matters, and we are left to consider any alternative views on our own. Might it be useful to distinguish a "Christian" from a "civic" strain in English humanism? Was its contribution all progressive, or did it do something to inhibit the native English genius? How far did it displace (or combine with) medieval ideas? The author asks few questions and (as a result) offers few answers.

Dowling has brought together some useful information about the literary and propaganda activity of Henry's reign, working from the top down. She shows that the king and his servants and several of his queens paid for the services of the new learning at court and in the schools and universities. She also offers a chapter on the "wandering scholars," which enumerates some of the English who went abroad and many of the foreigners who came to England. Her conclusion that these contacts show "the high esteem in which English scholarship was held" does not, however, seem warranted (p. 170). She makes no effort to compare the actual achievements of English and Continental humanists—Guillaume Budé, for example, with his English admirer, Cuthbert Tunstal. More seriously, she ignores whole areas of humanist activity, scholarly and literary, so that we learn almost nothing about humanist historiography and learning (for example, John Leland and Polydore Vergil) or the rest of the *studia humanitatis*: poetry, oratory, and moral philosophy. No doubt this is the proper territory of literary historians (such as C. S. Lewis) or historians of philosophy (such as P. O. Kristeller), but it does not help her book to ignore their work completely. She includes a late chapter on the new ideal of the gentleman, but without a single reference to Quintilian or the Renaissance Italians, and a final chapter on women and the new learning, although, admittedly, there is little to say on the subject.

Alistair Fox and John Guy, on the other hand, are not bashful about raising questions and prefer to argue rather than simply list, but they too offer few answers. Their book consists of a loose string of essays in three parts corresponding with their title: on humanism, politics, and reform. Fox begins with a salvo against his predecessors intended to show the confusion and inadequacy of past scholarship. He offers some useful caveats, particularly about the temptation to align humanism with a specific political or religious program, but his essay suffers from some of the same restrictions as Dowling's book: insularity, an arbitrary chronology, and a determined avoidance of the scholarly and literary side of the subject. Despite a plea to employ Kristeller's definition, his next two essays concentrate on the relationship between humanism and politics only to decide that Erasmus, Thomas More, Richard Pace, and Thomas Elyot offered nothing more than moral exhortations and pious hopes. Only Thomas Starkey is applauded for his political realism. Elyot particularly is castigated for his political equivocation and personal failure; no explanation is offered for the remarkable success of the *Book of the Governor* or his other contributions to the spread of humanism, presumably because they lie outside this deliberately narrow political perspective.

Indeed, politics is the real concern of this book, and the contrasting figures of More and Christopher St. German offer the main thread. Since Guy and Fox have already written biographies of both men, we seem to be getting here some random afterthoughts in which More continues to be derided and St. German elevated beyond their previous reputations. (Once again, we do not learn why More was read long after St. German was forgotten. Political realism would seem to have its limits too.) The best of these essays is probably Guy's on the king's council, which offers some help in understanding the political appeal of certain humanist ideas. Guy also supplies an essay on the intellectual background to Thomas Cromwell's reform ideas, modifying Geoffrey Elton's views somewhat by finding some antecedents for his revolutionary intentions. Some good things are scattered in these essays, but they do not really add up to a book. The bibliography at the end is even more selective than Dowling's and begins, like hers, with a meaningless list of unidentified manuscripts.

JOSEPH M. LEVINE
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JOHN J. MURRAY. *Flanders and England: A Cultural Bridge; The Influence of the Low Countries on Tudor-*

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Stuart England. Antwerp: Fonds Mercator. 1985. Pp. 401.

Thirty years ago the *AHR* (62 [1957]: 837–54) published an excellent article by John J. Murray entitled "The Cultural Impact of the Flemish Low Countries on Sixteenth- and Seventeenth-Century England." I thought then, and still believe, that this was a very fruitful line of research for development into an important book. Many of the paragraph topics suggested in the article have been greatly expanded in the current volume into detailed, almost encyclopedic, book-length chapters—on husbandry, economic development, science and technology, printing, language and literature, music, and the visual arts. In fact, Murray argues, there is hardly a field of English activity that the Low Countries did not influence. Out of hundreds of influential Dutch people, a few that caught my eye were Mattias Lobelius, who was in charge of the royal gardens under Elizabeth and James, Cornelius Vermuyden, who supervised the draining of the Great Level of the Fens, and Balthazar Gerbier, who outlined an early plan for a bank of England. But not just individuals helped establish the "cultural bridge" in the book's subtitle. Groups also added to the westward traffic: for example, the London Stranger Congregations greatly influenced the English Separatists in Elizabeth's reign. Although the author admits that refugee traffic moved in both directions across the "cultural bridge," he concentrates on only the westward stream. It would be interesting to look at Dutch Puritanism as well as English Arminianism, but that would be asking for a different kind of book.

The four chapters dealing with the religious impact of the Low Countries on early modern England claim the author's, and the reader's, special attention. Murray's pantheon of religious and intellectual heroes includes, among many others, such important Dutch figures as Erasmus, Arminius, and Grotius, and such important English figures as More, Milton, and Locke. According to the author, these men were part of a humanistic and Arminian intellectual tradition that gave rise to such liberal ideas as religious toleration. And there is much to be said, at least theoretically, for this thesis, especially since Arminian theology, with its tendency toward free will and good works, challenged the Calvinist doctrines of election and predestination. Recent scholarship on More, however, indicates that as lord chancellor he was in practice a determined persecutor of heretics. And, in England, Arminianism was closely associated, as the author briefly notes, with Archbishop Laud, the Star Chamber, and the Court of High Commission. Indeed, the Arminianism on both sides of the North Sea supported Erastianism—state control of the church.

Drawing on the work of W. K. Jordan, Murray believes that the battle between Presbyterians and Independents at the English Westminster Assembly in 1643 replicated the battle between Calvinists and Arminians at the Dutch Synod of Dort in 1618. But no Arminians (only Calvinists) were present at the Westminster Assembly. The author goes even further: "Only the English sectaries associated themselves with Dutch Arminianism" (p. 99). Analysis of most Puritan sectarians shows that their Antinomian ideas were at the opposite end of the religious spectrum from Arminianism. But, most curiously, Murray, despite a later disclaimer, states that ideas of religious toleration among the chaplains of the New Model Army "were more Arminian than Antinomian, more Netherlandish than English" (p. 92). Actually, the religious forebears of the Antinomians were such foreigners as Joachim of Fiore and Jacob Böhme as well as such Englishmen as Roger Brearley, John Everard, and John Eaton. A much better case for Arminianism in English religious thought might be made through the Quakers and the Cambridge Platonists, both mentioned only incidentally.

The Mercator Foundation has spared no expense in the breathtaking beauty of the richly colored illustrations that accompany the text of this oversize volume. There are gorgeous full-page reproductions of maps, portraits, books, paintings, landscapes, and various kinds of buildings. This volume will rightfully claim an honored place in any historian's library.

LEO F. SOLT
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HILDA L. SMITH. *Reason's Disciples: Seventeenth-Century English Feminists*. Urbana: University of Illinois Press. 1982. Pp. xx, 237. $18.95.

The setting of this book is the English Enlightenment, whose intellectual bounty, initially confined to men, had an unwonted effect on women. The disciples referred to are groups of women who began, in the late seventeenth century, to use their reason and powers of expression to identify the sources of their confinement to subordinate roles in English society. They asserted that women were intellectually equal to men and called for women's education; they also objected to the subordinate role women played in marriage and connected it with their impotence in the political and institutional life of England.

Hilda L. Smith has analyzed in a new way the works of eight female writers of seventeenth-century England, from Margaret Cavendish, Bathsua Makin, and Hannah Wooley in midcentury to Mary Astell, Elizabeth Elstob, and four "feminist"

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poets at the century's end. Smith notes, even stresses, the inconsistencies in the thought of these upper-class women, who, like Astell, understandably found it difficult to imagine the alteration in gender roles that logically followed from their protest. Most of them were Tory by background and Anglican by belief, which kept them from arriving at radical conclusions about marriage and family. They were also dependent, in a period of economic and social decline for women, on the support of men for both the leisure and the initial encouragement to develop their minds. Later in the century, however, poets such as Lady Chudleigh and Elizabeth Rowe presented more radical critiques of marriage and stressed the need for independence from men.

The women of this period placed great emphasis on female friendship and on the group identity of women. Smith points to this concept of women as a group, linked by their social and political position rather than by their physiology, in declaring these writers to have been the first modern feminists. This group-centered understanding, she holds, was "continuingly relevant" and "crucial to the development of a modern feminist chain of thought" that seeks social alteration of a group socially defined (p. 7). Even then, the growing understanding of women's common experience led to expressions of increasing bitterness about their situation from the poet Sarah Egerton as the eighteenth century dawned. The English revolution had come and gone. But, as cannot be emphasized too often, it had not been a revolution in behalf of the rights of all Englishmen, and certainly not of English women.

The feminist concepts uncovered by Smith were confined to the upper, educated classes, whose women may have had more in common with their French counterparts than the author admits, particularly in their recognition of the misogyny institutionalized in marriage and in their demand for education. Nevertheless, important factors isolated by Smith as feminist—awareness of women as a group whose status rises and falls together, connection of women's subordination in the family to that in society—were more highly developed in England. This accounts for their rejection of the method, used since Christine de Pisan, of citing great women of the past in their polemics.

Because the women covered in this work were of the educated classes, one might question their identification with all womankind. They could speak only to those who could read them, Smith concedes, noting that the same was true of liberal males at the time, for whom the rights of men referred to a minority of high status. Just as the broadening of Enlightenment views came to include the rights of all men, however, so the concept of women as a social group developed into the sisterhood of all women. It was too early for both in the seventeenth century. In fact, Smith indicates that the emphasis of female writers on reason, which placed them in the mainstream of their period intellectually, waned as the Age of Reason gave way to feeling in the eighteenth century, turning women from their argument for complete equality to an acceptance of separate roles. Yet, when feminism found its modern voice, it revived many of the points made by the "disciples of reason" in the seventeenth century.

This scholarly and well-written work, with footnotes providing a wealth of bibliographical information, provides valuable insight into an aspect of seventeenth-century England heretofore only superficially known and also makes a solid contribution to the history of feminism.

PHYLLIS STOCK-MORTON
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GERVASE MARKHAM. *The English Housewife: Containing the Inward and Outward Virtues Which Ought to Be in a Complete Woman; As Her Skill in Physic, Cookery, Banqueting-stuff, Distillation, Perfumes, Wool, Hemp, Flax, Dairies, Brewing, Baking, and All Other Things Belonging to a Household.* Edited by MICHAEL R. BEST. Montreal: McGill-Queen's University Press. 1986. Pp. lviii, 321.

This well-edited and attractive volume is a welcome addition to the growing body of scholarly work on English domestic life. We share the editor's hope that it will bring Gervase Markham's writings, particularly those dealing with cookery, to a wider audience. He is a valuable source for historians of social and economic life on both sides of the Atlantic, for he describes the domestic lore and practices that early settlers from England would have taken with them to the American colonies.

Apart from a few recipes from *The English Housewife* that turn up in histories of traditional English cookery, Markham remains little known outside the world of seventeenth-century scholarship. Soldier, scholar, poet, playwright, and horseman (he was said to have introduced the Arab horse into England), he had an early life full of incident and insecurity. Any ambitions he may have had as a courtier were dashed with the downfall of the earl of Essex, to whom the Markhams were allied. Bad luck and marriage reduced him to the precarious living of a farmer. The family appears not to have prospered on the land, but Markham used the opportunity to apply his scholarly talent to the study of husbandry and housewifery. The catalogue of his writings on these and other subjects, including equestrianism and country sports, is considerable, and they accurately reveal practices of the day that other writers largely ignored. As Michael R. Best

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remarks in his informative introduction, Markham's particular strength "was his ability to record the daily minutiae of the life he observed around him, not as a passive onlooker, but as one exploring and experimenting" (p. xvi).

The English Housewife is a compilation of material from disparate sources, although Markham often embellished them and occasionally added material from his own experience. The subtitle describes the wide range of subjects included. A religious man, Markham defined the ideal housewife as pious, clean, and frugal, "wise in discourse, but not frequent therein" (p. xxiii). Although he valued a measure of retirement in women, he expected full participation from them in the domestic economy. This is a point brought home by Best. Whether supervising the kitchen and the dairy or preparing homemade remedies, the practical English housewife in the seventeenth century had a lot on her plate. Her more prosperous nineteenth-century counterpart delegated more of the responsibilities of the kitchen to servants and bought proprietary medicines from the druggist. But the price to be paid for this was a decline in English cookery and the greater relegation of middle-class women to ornament.

Markham recognized the important relationship between diet and health, and, although we cannot recommend most of his medicines, many of his dishes look delicious. They represent the traditions of English country cooking, as opposed to the "courtly" style of France. Markham, like Charles Carter and Hannah Glasse a century later, was hostile to French culinary pretension. One of the aims of this edition is to tempt readers to cook some of his recipes, but, sadly, most will be put off by its academic format and the lack of specific guidelines. We have spent long hours cooking from an eighteenth-century manuscript with dishes not unlike Markham's and can attest that there are great rewards in tasting the distinctive flavors and textures of traditional English cooking from a period before French cuisine swept all before it. It would be a welcome result of this fine edition if an adventurous publisher commissioned a popular book of a selection of Markham's recipes translated for the modern kitchen.

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RICHARD ASHCRAFT. *Revolutionary Politics and Locke's* Two Treatises of Government. Princeton: Princeton University Press. 1986. Pp. xxii, 613. Cloth $65.00, paper $15.00.

Richard Ashcraft's reinterpretation of John Locke's *Two Treatises* is the most significant work of the past decade in Locke studies, Restoration political scholarship, and the history of seventeenth-century political ideas. As a member of the "family" of the earl of Shaftesbury, Ashcraft's Locke was the premier theorist of the radical opposition movement to Charles II that became the core of the exclusionist Whigs. The arguments of both *Treatises* accordingly arose from their writer's deep personal and intellectual engagement in the political and religious disputes of the Restoration that matured in the Exclusion Crisis. For this reason, the author suggests, ahistorical commentators who have treated the *Second Treatise* as an abstract work detachable from the circumstances of its composition have misread Locke. So has C. B. Macpherson, who sought to link Locke and the Levellers as liberal apologists for property holders. What Locke and other radical Whigs shared with the Levellers was not moderate constitutional revisionism but a revolutionary commitment to social contract theory that sanctioned a broad parliamentary franchise and a popular right to resistance. According to this perspective, Charles's obstruction of Parliament constituted a dissolution of the government, a forfeiture of royal authority, and an invasion of the natural rights of the sovereign people. Such arguments were made in the early 1680s by Whig leaders, who addressed artisans, shopkeepers, merchants, and small gentry in a "social language" developed more philosophically in the manuscript of the *Second Treatise*. Interpretations of Locke's chapter about property as politically restrictive are, therefore, dismissed by Ashcraft, who distinctively argues that the chapter is an outspoken "class-oriented" defense of the political rights of the Whiggish "sober and industrious" sort.

Ashcraft also establishes that the philosopher's radical words were expressed in revolutionary deeds. Associated with leading Rye House conspirators, Locke made notable contributions in his Dutch exile to Monmouth's ill-fated rebellion, and Locke was among those radicals who thereafter persisted in conspiracy. When the revolution of 1689 proved a disappointment, Locke published his *Treatises* to sustain revolutionary élan. Not an apology for the revolution, his work instead indicted it as a philosophical and revolutionary charade.

Among the diverse accomplishments of Ashcraft's book are three important contributions to late seventeenth-century political historiography. First, he treats the Exclusion Crisis as a real crisis with revolutionary potential rather than as an amateurish dress rehearsal for the more glorious revo-

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lution to follow. Second, by placing Locke's political theory amid the polemical literature of the 1670s and 1680s, Ashcraft reveals an ideologically richer and more varied Whig opposition. Third, demonstrating radical continuities between the Levellers and exclusionist Whigs, he also illuminates the shadowy underground world of radicalism in London and the Dutch republic.

A work of such breadth and argumentative vigor is bound to provoke questions and debate. For instance, Ashcraft develops his study on the contextual dogma that any political theory can be understood only in relation to its historical and sociological origins. Agreeable to social historians of ideas, this approach continues to strike other intellectual historians and some students of political philosophy as being less universally applicable than Ashcraft claims. Although certainly a persuasive case study in the relationship between one body of political theory and the meaning of corresponding social and political action, the book is not without historical fault.

First, Ashcraft's discussion of the social audiences for radical Whig thought is marred by imprecise terminology. Are "tradesmen," for instance, to be considered as part of a "class of artisans, tradesmen, laborers," and almsmen (p. xiii) or as a distinct class with interests allied to those of another class of merchants (p. 242)? Because Ashcraft offers little definition of the concept of class, using it generally as a synonym for "socioeconomic group" (p. 228), "class-oriented" radical arguments lose their edge. Second, Ashcraft's belief that radical Whig ideas had strong appeal among virtually all urban social groups skirts political tensions among those groups, identifies Tory principles too narrowly with landed and clerical interests, and derives from dated secondary sources (pp. 247, 252, 281, 578–79). Third, given the importance attached to dissenters among radicals, the statement that dissenters made up "a very small minority of the total religious population" is a surprising and simplistic generalization (p. 501). Fourth, in treating London as a radical Whig stronghold, Ashcraft makes erroneous statements about the City's parliamentary franchise (p. 148) and the extent of Whig "control" of its political institutions (p. 345). Fifth, the author has agreed with Locke about the relative absence of "official" radicalism in 1689 without responding to some recent arguments for a more radical revolution.

These reservations aside, Ashcraft has successfully overcome the conventional disciplinary preoccupations that impair the study of politics.

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WILLIAM ROOSEN. *Daniel Defoe and Diplomacy*. Selinsgrove, Pa.: Susquehanna University Press or Associated University Press, London. 1986. Pp. 144. $23.50.

This book is a self-destructing artifact. It contains within itself unmistakable indications that it should not have been written: Defoe "had few original ideas" (p. 16); he "did not create a fully developed . . . theory of international relations" (p. 22); he "never organized his thoughts about diplomacy" (p. 87). Ignoring these storm warnings, William Roosen sails boldly out to sea collecting Defoe's scattered obiter dicta as though Defoe were another Hugo Grotius. This leads to some conclusions of numbing banality: Defoe "argued . . . that things must be taken as they are" (p. 23); "Defoe maintained separate sets of ideas on different topics at the same time" (p. 29); "he understood . . . that the strength of the whole is greater than the sum of [the] parts" (p. 63). Defoe's obiter dicta on diplomacy involve a large element of contradiction: "Defoe once favored open diplomacy . . . Defoe argued that diplomatic negotiations had to be kept confidential" (p. 61).

Some of this contradiction seems to seep into Roosen's text: "Defoe appears not to have been very interested in foreign affairs . . . Diplomacy and international affairs were undoubtedly very important to Daniel Defoe" (pp. 20, 96). That word "undoubtedly" and its disreputable cronies "perhaps," "may," and "probably" (p. 68) have bedeviled Defoe studies for more than fifty years. But the reader is not to worry about contradictions in Roosen's text, for "one characteristic of international relations is that, in practice, statements that are apparently contradictory can both be true" (p. 98).

In constructing this book Roosen deploys all the arts by which the big book that Samuel Johnson talked about is made. Eighty-six pages of text are embedded in fifty-eight pages of front matter, chronology, end notes, bibliography, index, and six black-and-white (but murky) illustrations. Since it was written for historians, who can be introduced to a king of Spain called "Charles III" and to "the Görtz-Gyllenborg affair of 1717" without further ceremony (pp. 24, 32), it is difficult to understand why twelve pages of chronology are needed. It is equally difficult to understand why 440 end notes are needed for eighty-six pages of text. Even a rhetorical question on page eighteen has its own end note.

Roosen seems to catch glimpses of the fact that Defoe was a publicist (pp. 31, 68), writing "to persuade readers to accept [the] viewpoint or interpretation of events" of others (p. 77). Yet Roosen's method splits off Defoe's publicity from the events publicized. But, once Defoe's publicity is restored to its historical context, many of the contradictions are resolved. In 1712, to cover up the separate and

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secret negotiations between England and France that preceded the Treaty of Utrecht (or because he did not know of these negotiations), Defoe advocated open covenants openly arrived at (p. 61, n. 26). In 1717, to defend Oxford, imprisoned in the Tower for negotiating the Treaty of Utrecht, Defoe advocated secret negotiations as the only way to achieve the compromises essential to treaty making (p. 61, n. 29).

Roosen mentions that Defoe never held any official diplomatic position (p. 96). But this calls attention to the fact that Defoe held one unofficial diplomatic position of extraordinary importance. From September 1706 to December 1707 he was a secret agent, propagandist, provocateur, and arbitrator for the English government in Edinburgh during negotiation of the treaty of union with Scotland. Defoe's response to this challenge has been called "one of the most remarkable propaganda feats in history." In the present context it can be called one of the most remarkable diplomatic feats in history. But of this feat there is not a word in Roosen's book.

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JEREMY BLACK, editor. *Britain in the Age of Walpole*. New York: St. Martin's. 1984. Pp. 260. $27.95.

"Someone," E. P. Thompson wrote twelve years ago, "must have benefited from Walpole's administration; although having read most of the state papers and most of the surviving newspapers for the years 1722–24, I am at a loss to know who this was, beyond the circle of Walpole's own creatures." Three chapters of this volume of collected essays offer perspectives on this circle: Evelyn Cruickshanks's on Robert Walpole's political henchmen; J. A. Downie's on who became a "hired" journalist and why; Michael Harris's on the persons who distributed printed propaganda. In these cases the regime's largess seems to have been attentively matched to the services rendered. But of course the main question is whether anyone "beyond the circle" reaped any benefit. Because the contributors are disposed to approach this question without prejudice, the book opens avenues to an answer. Downie, in fact, usefully addresses the picture of regime-induced general decay that was painted by opposition writers (and still remains influential) as a curious phenomenon.

Positive benefits and beneficiaries "beyond the circle" are, it must be granted, not readily discernible. Walpole's quest for quietude and his steady emphasis on preservation of power are central facts. Public benefits, such as they were, arose from the mere persistence of stable government. Moreover, Walpolean inertness was probably beneficial to the populace as a whole, at least in England and the American colonies. Whether neglect was salutory in Scotland and Ireland is more problematic. Scottish politics at this time would appear to deserve the "banana-republic" label more than English, and the north British weeds of corruption had vigorous native roots, as Bruce Lenman's tart portrayal reveals. But he also shows that the Walpolean system manured the ground. David Hayton concludes that in Ireland Walpole casually sacrificed measures to men—never mind the Jacobite danger when it came to Ireland, evidently—and even sacrificed the regime's network of men in Ireland to English political priorities.

The book as a whole conveys a strong impression that pursuit of quietude did not mirror the complacency of a secure "Venetian Oligarchy." (Maybe that came later, at a point farther removed from seventeenth-century turmoil.) It mirrored instead an uneasy feeling of being precariously near the brink of instability and a pervasive sense of political limits. The task of governing without reliable instruments of repression, censorship of the press, or divine-right mystique was unfamiliar and arduous. It provided an ample public agenda for the allegedly "do-nothing" regime, as H. T. Dickinson's wide-ranging essay on popular politics deftly reminds us. The importance of the public aspect of the regime's activity is implicit in the essays by Cruickshanks and Downie and also in parts of Michael Jubb's survey of economic policy. All in all, attention is shifted away from Namier, Inc.'s—I borrow Lawrence Stone's term—overriding concerns (manipulation of elections and aristocratic allegiances) toward a much larger political arena.

No claim can be made that political success hinged on "issues" in Walpole's time, except in the negative sense that the regime was wise in classifying some issues as untouchable. There were, however, a few that could not be kept asleep, ones that we today, knowing the outcome, have difficulty viewing with the same anxiety that worked on Walpole. Among these were Jacobite disaffection, the supposed fragility of public credit, and the repercussions of an expensive or unpopular foreign policy.

Regarding the last, Jeremy Black's essay makes a forceful contribution. Black, whose knowledge of British foreign policy in this period is unrivaled, claims that Walpole's noninterventionism in the 1730s was vindicated by subsequent events. He also shows that the "Hanoverian foreign policy" that the populace detested was objectionable not only because of chauvinist-isolationist sentiment but also because it often obstructed the best diplomatic and strategic choices. Walpole opposed the European activism of the monarchs as much as he could, and his strong inclination toward preserving peace was a

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boon to the middling and lower ranks of society on whom war in those days pressed most severely.

This book is incorrectly titled or at least should have been subtitled "Politics and Policy." Considerable progress on the subject of stability in the age of Walpole is likely to come—has already started to come—from studies conducted outside the canonical purviews of political history. Those purviews stand in the penumbra of this book. But its title reflects a seemingly unconquerable presumption that clings to the history of eighteenth-century Britain, namely, that its political history encapsulates its history.

DANIEL A. BAUGH
Cornell University

W. F. BYNUM and ROY PORTER, editors. *William Hunter and the Eighteenth-Century Medical World*. New York: Cambridge University Press. 1985. Pp. xi, 424. $49.50.

This is an exceptionally rich collection of essays in the cultural history of medicine loosely organized around the life, interests, and professional milieu of the late eighteenth-century surgeon, anatomist, and "male midwife" William Hunter.

The book is divided into four sections. The first is meant to deal with Hunter's life but in fact offers much more. Roy Porter's piece gives a witty and immensely informative account of the entrepreneurial, theatrical, and of course more narrowly professional aspects of succeeding in the London marketplace of learning. C. Helen Brock's essay on Hunter's financial fortunes demonstrates how much one can learn about a profession by studying the minutiae of its economic life.

Four of the book's fourteen contributions deal with medical education in Great Britain: Joan Lane's on apprenticeship, W. F. Bynum's on the role of hospitals in the career structures of eighteenth-century physicians, Toby Gelfand's on the business of teaching in a charity hospital, and Christopher Lawrence's on what made the University of Edinburgh so attractive to medical students. Two essays—Johanna Geyer-Kordesch's on the cultural and clinical context of German, specifically Prussian, education and Othmar Keel's survey of European clinical practices—offer Continental comparisons. Geyer-Kordesch's contribution is a model study of the interaction between institutions and medical theory, on the one hand, and seemingly extraneous beliefs—Lutheran Pietism in this case—on the other. As a group these essays constitute a cautionary tale against viewing the history of medical education solely through the prism of Parisian hospital clinics.

Next come two pieces in the high intellectual history of medicine, both dealing as much or more with John Hunter than with William. Francois Duchesneu gives a survey of the vitalistic theory of Paul-Joseph Barthez, Johann Friedrich Blumenbach, and John Hunter; W. D. Ian Rolfe recounts the Hunters' work as natural scientists concerned with various aspects of the "great chain of being" and its demise. Both essays are conservative in their methodology; both are lucid and learned.

The book ends with four essays on obstetrics. Angus McLaren surveys eighteenth-century theories of conception, Adrian Wilson and Edward Shorter describe the practice of "man-midwifery," and Ludmilla Jordanova gives an extraordinary reading of Hunter's *Anatomy of the Human Gravid Uterus*. Medical illustration is not, Jordanova makes clear, simply a matter of accurate presentation. There is first of all an epistemological claim being made in a book such as Hunter's: that seeing is knowing and that an unmediated representation of corporeal reality is possible. Realistic techniques such as rendering hair, facial features, and other nonessential details with great precision create the illusion that what one sees is truly what there is. Through formal aspects of representation Hunter thus made a claim for anatomy very different from that of his contemporary, S. T. Soemmerring, whose equally beautiful illustrations argued for the existence of ideal forms. The role of the anatomist for Soemmerring is thus to exclude defects and details and produce a prototypical and ideal organ or structure.

Moreover, Jordanova shows how anatomical illustration links the power implicit in the anatomist's conquest of death and disorder with the overt content of Hunter's obstetrical plates. Women's privy parts are there to be seen and known by male doctors. Seemingly neutral pictures of the gravid uterus thus reinforce and indeed help create the image of woman as nature and object of knowledge whereas the doctor alone is subject and capable of knowing. Jordanova forces us to question what Hunter assumed was self-evident and natural. There is a graininess and intellectual vigor in this confrontation that make Jordanova's piece the most exhilarating and unsettling of this fine collection.

Specialists will find much of use here: up-to-date references, much new material in text, tables, and appendixes, as well as clever revisions of received views. But it would be a pity if this book fell only into the hands of medical historians. Its cumulative point is that medical history is part of social and cultural history generally, that it is as powerful a lens onto how the world works and how texts produce meaning as any other form of history. My students have already found several of the essays invaluable, which

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suggests that a cheaper, paperback edition might well find a market.

THOMAS W. LAQUEUR
University of California, Berkeley

ERIC KERRIDGE. *Textile Manufactures in Early Modern England*. Dover, N.H.: Manchester University Press. 1985. Pp. ix, 428. $50.00.

Readers of his work on early modern English agriculture will recognize in this book Eric Kerridge's characteristic attributes: the detailed account, grounded in an array of primary and secondary materials, and incorporating a polemic against scholars whose socialist sympathies have allegedly deformed their historical judgment. Previously, R. H. Tawney drew Kerridge's fire; in the present book George Unwin is rebuked for positing exploitation, class division, and social conflict.

The geographical distribution of textile crafts, allied trades such as hosiery and lace, the sale and preparation of fibers and dyestuffs, the general if slow tendency to mechanization in spinning and weaving, the organization of production, and the sale of finished cloth in England and abroad each receive a chapter. But the core of the book consists of a valuable inventory of the development of various categories of woolens and other cloth. A glossary of technical terms would have been helpful, as would graphic presentation of the scattered quantitative data, but Kerridge excels at explicating weaving processes, and the dense text is enlivened by information about the English names of many kinds of cloth.

Although accepting that some fabrics were introduced by immigrants, Kerridge insists that the success of English textiles should primarily be credited to indigenous innovation. At times, his thesis runs ahead of the evidence, and his opening chapter, which places the medieval English textile industry within the European context, shows that rapid diffusion of innovation had long been the norm among all European textile producers. Besides, scholars such as G. D. Ramsay have cautioned us not to overemphasize the novelty of early modern fabrics, given both the interchange between English and Continental clothmakers and the long tradition of worsted weaving throughout England.

Kerridge attributes regional specializations to the local availability of wools, dyestuffs, millstreams, and other natural factors. Surprisingly, he neglects the evidence provided by Joan Thirsk, Eric Jones, and other scholars of links between agrarian structures and rural industry, and the proto-industrialization debate is absent from his pages. Consequently, he offers little explanation of why particular textile-producing regions rose and fell, other than an asserted proclivity for centrifugal diffusion of older cloth types to remote areas, at times offset by the adoption of new kinds by established locales.

In the book's longest chapter, Kerridge disputes historians who interpret the rise of the textile industry as eventuating in the emergence of merchant entrepreneurs managing putting-out systems. Although acknowledging some structural change of this sort, particularly in rural areas where a limited assortment of cheap, simple cloth was being produced, Kerridge maintains that even there most producers were independent contractors. In the towns, he claims, the domestic system was little developed: even the introduction of new techniques and fabrics enhanced the role of masters who built up substantial shops on their own premises.

In some respects, Kerridge aims at strawmen, for since Unwin's time scholars have come to recognize that big clothiers represented only a minority of producers and that organizational structures varied greatly among regions. Still, Kerridge's argument about the continued vitality of production controlled by masters is important and connects with a developing trend of interpretation that rejects any single structure as having encompassed preindustrial production.

For Kerridge, the triumph of English textiles in the face of fierce competition is to be credited to the unleashing of entrepreneurial genius whose constant innovations were secured by the rule of law and firmly guaranteed property rights. Yet his discussion of these factors is too sketchy to carry complete conviction, as is his rosy interpretation of wages and working conditions. For all its interpretive shortcomings, however, this book constitutes the best available compilation of technical matters and clearest typology of the English cloth trades for the period preceding the Industrial Revolution.

ROBERT S. DUPLESSIS
Swarthmore College

J. M. BOURNE. *Patronage and Society in Nineteenth-Century England*. London: Edward Arnold. 1986. Pp. ix, 198.

J. M. Bourne attempts something of a revision in nineteenth-century English history by arguing that "patronage permeated English life" (p. vii). Rather than being "neglected and dismissed as an unimportant antiquarian interest, a mere appendix to the body politic," Bourne insists that patronage should command a great deal of respect and attention because of its "pervasiveness, diffuseness and diversity" (p. viii). Bourne's is only "something of a revision" because few would deny the continuing existence of patronage in the period. Quite the

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contrary, a number of careers have been constructed on similar assumptions, mixed inextricably with the issue of deference. The question concerns the degree and the duration of the continuity. Bourne's conclusions about the beneficiaries of nineteenth-century patronage are more genuinely revisionist since he argues that the middle classes, particularly the London-based, nonindustrial middle classes, were most often found on the receiving end of the system.

Despite such sweeping claims about the nature of patronage, Bourne recognizes the difficulty encountered in assessing such a nebulous phenomenon. Bourne begins, therefore, by trying to define patronage with sufficient clarity to allow analysis and by identifying the sources he believes permit close study. Even more than most historical arguments, the success of Bourne's task depends on his definitions. Although he argues reasonably enough about the nature of patronage and the surviving traces of it such as "begging letters," he nevertheless succumbs to the fuzziness of the concept. Eventually, despite the elegance and sophistication with which he writes about this remarkably complex topic, Bourne presents an unfalsifiable and therefore unacceptable thesis. According to his definition of patronage, any country at any time would be vulnerable to the charges he levels. A good example of his penchant for counting virtually anything and everything as patronage is his inclusion of Victoria's coronation as Empress of India as "the most stunning example of prime ministerial patronage in British political history" (p. 73). Perhaps Benjamin Disraeli's act warrants such a characterization, but it hardly seems indicative of anything, nor does Victoria's own involvement in the creation of the Victoria Cross. To count recipients of VCs as beneficiaries of patronage is to stretch the term beyond useful meaning. Not content with this, Bourne eventually also includes those on the western front who won Military Medals and Military Crosses. Bourne's attitude toward both nineteenth- and twentieth-century Britain is reminiscent of Anthony Sampson's, but his demonstration is less persuasive.

The anecdotal evidence of patronage that Bourne offers for consideration is interesting, certainly, and often underscores important facets of Victorian society, but such an inherently indeterminate concept requires much more rigorous handling if its impact and general nature are to be assessed. Bourne follows a traditional, narrative approach that undermines much of the potential of his study. He ignores even the most fundamental statistical considerations and makes obviously specious arguments. For example, by presenting just the raw number of people employed in the metropolitan and provincial departments of central government between 1797 and 1914, Bourne argues that, though "hardly dramatic," "the general trend is clearly upward" (p. 22). The raw numbers of office-holders did generally increase over the period, but the growth of central government offices only kept pace with the growth of the population between 1797 and 1881. The real explosion in the number of offices (measured proportionately) occurred in the twentieth century. Similarly, Bourne highlights what even he admits were "modest additions" to the orders of chivalry even though the numbers in question did not keep pace with the burgeoning population. Only with the introduction of the Order of the British Empire in 1917 did a substantial expansion occur. Some sense of proportional balance is no less important than evidence of absolute increase. Without both, any account is likely to be misleading. This approach renders Bourne's account of patronage fascinating but not definitive. One is left unconvinced about how pervasive patronage was, both absolutely and relative to the "patronage system" of the eighteenth century.

JOHN A. PHILLIPS
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HEW STRACHAN. *From Waterloo to Balaclava: Tactics, Technology, and the British Army, 1815–1854*. New York: Cambridge University Press. 1985. Pp. xi, 188. $34.50.

In contrast to the typical monograph about the supposedly unreformed British army after Waterloo, this study by Hew Strachan demonstrates that the infantry, artillery, and cavalry were significantly modified in forty years. In Strachan's *Wellington's Legacy: The Reform of the British Army, 1830–1854* (1984), he studied the administration of the early Victorian army, and he concluded that major transformations in the theory and practice of war were underway. The author's second book on the same theme complements his earlier work. Here, Strachan covers a broader chronology; he analyzes the major branches of the army and provides abundant details about experiments in training, tactics, and weaponry that occurred under the impact of technology.

In the pre-Crimean era the army appeared virtually to ossify because of budgetary restrictions, the long decades of peace, and the dominant influence of the duke of Wellington. But, as the author shows, veterans of the Peninsular War and of colonial wars in India and the Cape Colony urged improvements of small arms, ammunition, line formations, and both light and heavy artillery. The future role of the *arme blanche*, generally ignored by Wellington for offensive actions in Spain, was likewise debated. Pivotal to these discussions was the fact that the

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army had several functions—to suppress violence at home, to prepare for a possible invasion by France, to campaign on the Continent, and to police an ever-expanding empire.

Thus, what changes occurred in the infantry? Field manuals were revised, light troops were used like skirmishers, and a more reliable bayonet was acquired. More attention was devoted to marksmanship, to standardizing weapons, and to finding a dependable breach-loading rifle. The adoption of the Minié rifle increased the range and accuracy of the line as commanders sought the precise combination of mobility and firepower for the battlefield.

The cavalry underwent less significant changes. In the Peninsular War, the horsemen's charges had frequently been blunted by infantry squares. Hence, there were frequent tests with better lances, swords, sabres, pistols, and carbines. Gradually, the former distinctions in the use of dragoons, light horse, and heavy brigades became blurred. Long before the trench warfare of World War I, the value of cavalry as shock troops seemed limited for European conflicts, but in the Sikh Wars and other colonial campaigns the horsemen proved invaluable.

In the artillery, gunners received longer training, wheels and carriages were bettered, and constant experiments were held to improve the range and accuracy of coastal artillery, siege cannon, and field artillery. By the 1850s the size, weight, and destructive power of the guns had increased greatly. Thus, under the influence of contemporary military thought and technological innovations, the author concludes, the British army was modernized.

The author merits high praise. His eighteen pages of footnotes—much of it fresh material—attest to his scholarship. The prose is readable, the bibliography excellent, the index serviceable, and the organization of this difficult topic admirable.

RICHARD BLANCO
State University of New York, Brockport

MARTHA VICINUS. *Independent Women: Work and Community for Single Women, 1850–1920*. (Women in Culture and Society.) Chicago: University of Chicago Press. 1985. Pp. xv, 396. $27.50.

In comparing the introductions written by Martha Vicinus for *Suffer and Be Still* (1972) and for *Independent Women* (1985), it is gratifying to note that the validity of women's history as an academic discipline no longer needs to be justified. Historical inquiry using gender as a perspective is now acknowledged in the same way that class analysis has been. In her most recent work on the achievements of middle-class single women in the nineteenth century, Vicinus blends in a stimulating way these two historical tools and also traces the opposition faced by nineteenth-century women who sought a role in the public sphere.

In this book Vicinus maps the path that single women took in their quest for autonomy and women's rights and ends with a description of the suffragettes' efforts. Vicinus shows that, in moving the argument away from one of what to do with surplus women, women created an "honourable independence" for themselves (p. 162). She focuses on women's daily life in work-based institutions as a way of understanding the unmarried middle-class woman in the nineteenth century. She examines sisterhoods and deaconesses' houses, reformed hospital nursing, women's colleges, reformed boarding schools, and settlement houses and thus covers the areas of public life deemed respectable for women. We "see the development of leadership skills, friendship networks, and a power base for public work" by women (p. 7). She does not portray the women or their institutions statically but examines the generational differences between women born in the 1840s and 1850s and those born in the 1870s and 1880s as well as the differences between a pioneer such as Florence Nightingale and those who came after her.

Vicinus rightly claims that women's communities were a liberating factor in women's lives yet contained seeds for failure. In structuring their communities, women understandingly adapted the family as a model (p. 185). The notion that women had a special temperament for nurturing that allowed them to undertake certain responsibilities in the public sphere was tied to this familial base. As Vicinus points out, this "had been an attractive notion, given women's lack of an alternative power base" (p. 289). Still, this view did not completely put women in the public sphere, nor did it help them develop a strong economic base because "it failed to confront the degree to which sexual differences were structural and social rather than personal and moral."

This book has scholarly merit for many reasons. It is indeed a sympathetic and comprehensive treatment of the effects that the rigid separation of spheres had on middle-class women's attempts to enter the public sphere and obtain economic and political rights. It makes strong connections between women's role in the family, how that role was affected by religion, and how family and religion influenced a particular class of women in the world of work. In addition, while offering a historical narrative focusing on independent women and their work in the nineteenth century, Vicinus also provides generalizations about the perceptions of women's place in Western society. The deep-seated fear of women in European culture, which she notes (p. 31), needs to be evaluated not only in consider-

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ing the decline of women's communities after World War I but also in reflecting on the difficulties faced by the contemporary women's movement and the fact that women are usually assistants whereas men are usually policy makers. Her timely historical analysis of gender and class constrictions make this book significant.

ELLEN F. MAPPEN
Douglass College

REGENIA GAGNIER. *Idylls of the Marketplace: Oscar Wilde and the Victorian Public*. Stanford: Stanford University Press. 1986. Pp. 255. $29.50.

This brief literary monograph offers a striking reinterpretation of the works of Oscar Wilde and of late Victorian aestheticism in general. Instead of seeing Wilde as merely the popularizer of art-for-art's sake or as the spokesman for formalist theories of art, Regenia Gagnier focuses on Wilde's relationship with his audiences to illuminate the position of the artist in a consumerist society—a "society of spectacle." Her analysis seems to draw on reader response theory as well as cultural history. It enables her to deepen our understanding of Wilde's apparently contradictory work and to suggest that aestheticism was not a doctrine of artistic isolation but "an engaged protest against Victorian utility, rationality, scientific factuality, and technological progess" (p. 3). The result is a book full of brilliant insights for intellectual historians as well as literary scholars but one that is also unnecessarily elusive and opaque.

Gagnier argues that Wilde was naturally a man of letters, a writer with acute political instincts; however, the world of high culture no longer accepted the authority of broad-ranging sages. Academic specialization and the rise of mass journalism made that role impossible. Wilde also recognized that because of the decline of the aristocracy and the rise of consumerism there was an exceptional fluidity in fashionable society. Thus, in his social and aesthetic theory, as in his literary works, Wilde sought both "to meet the challenge of the general public and . . . to attract a special audience" (p. 4) of artistic young men by developing two prose styles: one clever and epigrammatic, meant to criticize as well as to divert the general audience; the other "jeweled and seductive," meant to create a private audience of the select few.

Both Wilde's *Picture of Dorian Gray* and his stage comedies were about the relationship of art to life. As Gagnier points out, Wilde never wrote a story without a moral. Yet the critics found *Dorian Gray* offensive because its action took place in an aesthetic rather than a bourgeois environment. As for his public image, Wilde adopted dandyism as a form of self-advertising to take advantage of consumerism while simultaneously criticizing the standard idea of the gentleman. In his dramas Wilde dissolved the line between life and art so as to manipulate the glittering play-going society. Gagnier contends that Wilde saw fashionable society as a spectacle—a "self-portrait of power" (p. 126). Consequently, he presented his characters in their splendid settings as the very self-image of the audience. He provided sentimental plots to allow the audiences to feel good about themselves but exposed their hypocrisy by his cynical dialogue. The critics, who disliked Wilde's plays, missed the point, which was to illuminate the universal posing in a society revolving around power and commodities.

Society took its revenge on Wilde by confusing his aestheticism with his homosexuality. In his works and in his life, Wilde had criticized Victorian purposefulness and productivity. Thus, according to Gagnier, Wilde was attacked and convicted as much for his values as for his homosexuality. And, in attacking him, the public linked artistic genius with abnormality. The solitary confinement of prison life threatened to drive Wilde insane, but he saved himself by writing *De Profundis*, a work shaped by the very absence of an audience.

This is an extremely interesting and stimulating book, but it is not consistently clear in either its prose or its theoretical concepts. It complexifies and enriches the intellectual history of Oscar Wilde, and it succeeds in reconciling his sentimental and cynical sides. But the claim to have illuminated aestheticism in general assumes an identity of Wilde with the whole movement. Moreover, the passages analyzing the "spectacular" society ought to be pulled together and explained systematically. As they stand, the author's allusions to cultural theories are intriguing but not persuasive.

THOMAS WILLIAM HEYCK
Northwestern University

KEN FULLER. *Radical Aristocrats: London Busworkers from the 1880s to the 1980s*. London: Lawrence and Wishart. 1985. Pp. 256. £4.95.

Today it is difficult to imagine busworkers as an aristocracy of labor since the prospect of navigating a double-decker bus through London seems to be unappealing even though Britain has a nagging unemployment problem. During the 1950s and 1960s London Transport was forced to recruit workers in Barbados and Malta, employ women, and resort to one-person operation of buses. It was a far cry from 1927 and 1928 when less than two thousand men out of sixty-one thousand applicants were actually hired.

Ken Fuller's history must be recognized as the trade equivalent of an authorized company history.

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Fuller was a bus driver for eleven years and now is a full-time official in the London Bus Section of the Transport and General Workers' Union (TGWU) and confesses that the purpose of the book is to assist members of the London Bus Section in rediscovering their militant heritage. As a result events are rarely portrayed from any but a left-wing, militant point of view.

Fuller begins with a patchy treatment of Thomas Sutherst's New Unionist organization founded in 1889 and does not really warm to his subject until he discusses the "Red Button" busworkers' union, which contained elements opposed to World War I. He next describes its amalgamation with the TGWU under Ernest Bevin's right-wing leadership before turning with relish to the violence in the General Strike. The lion's share of the book is devoted to the rise of the Busmen's Rank and File Movement and its defeat during the Coronation Strike of 1937. The National Passenger Workers' Union, an antipolitical union active from 1938 to 1947, though syndicalist in origin, fails to pass Fuller's litmus test of militancy and receives decidedly negative treatment. The final third of the book deals with the busworkers during and after World War II. Their decline is partially attributed to Deakinism and the threat of mass car ownership. Fuller then lauds the impact of Frank Cousin's left-wing leadership of the TGWU and analyzes the unsuccessful bus strike of 1958. Here the book is flawed because it traces related events and personalities in two separate chapters dealing with the busworkers' loss of status and forms of struggle, which makes understanding cause and effect difficult.

Yet the student of industrial relations can learn much from this work. Scattered here and there are admissions of flawed union policy: numerous counterproductive wildcat strikes, failure to forge stronger links to the Underground's rail unions before the 1958 strike, the union's antagonism to tram workers, and its initial reluctance to champion higher wage differentials for one-person crew drivers. These flaws reveal an insular arrogance that is echoed by Fuller's own Luddite sneers at the automobile and "consumerism." He gives cavalier treatment to union officers who volunteered for World War I and glibly tries to put a good face on the union's early discriminatory policies regarding race and sex. Bevin emerges as the villain of the piece as the adversary of the unselfish, left-minded George Sanders and Bill Jones, who remained a member of the Communist party until after the Hungarian Revolt. Although Fuller uses a wide range of sources, his judgments are generally made on the basis of "truth" gleaned from publications of splinter movements and reminiscences from left-wing, militant survivors.

Fuller's conclusion bravely grapples with the question of what policy trade union members in declining industries ought to follow. The British experience demonstrates the limitations of close affiliation to a political party that on assumption of power might of necessity become an adversary rather than an ally. His fears of privatization ring hollow after his complaints about national and county council monopolies. The commuter wants a cheap ride, whereas busworkers seek wage justice and improved working conditions. Can syndicalism keep the trade union bureaucracy from excessive accommodation? What is to be done? Fortunately, even with the decline of riders and the abolition of London Transport and the Greater London Council, the busworkers, as members of a semidispensable sector of the economy, will not go the way of the tramway operators, but, like the landed and labor aristocrats before them, they will have to adjust to a redefined status.

NORBERT C. SOLDON
West Chester University

BERNARD SEMMEL. *Liberalism and Naval Strategy: Ideology, Interest, and Sea Power during the Pax Britannica*. Winchester, Mass.: Allen and Unwin. 1986. Pp. xii, 239. Cloth $34.95, paper $14.95.

Bernard Semmel's reputation rightly rests on his exposition of British political thought in the late nineteenth and early twentieth centuries concentrating on the relationships of liberalism and socialism to imperialism and foreign policy. Thus, his interest in commercial and naval strategy is natural. He has read widely, including Admiralty papers in the Public Records Office. Nevertheless, this is essentially intellectual rather than strategic history. It pays little attention to the technological developments that compelled changes in strategy and tactics, although Semmel is aware that others are working on these issues. He is not concerned with naval officers and what made them think. He brings them into the story, perhaps mistakenly, for he assumes that the reader has read Donald Schurman, Arthur Marder, and Paul Kennedy.

Essentially, the approach is to sweep through the subject in an all-encompassing manner in which quotations are liberally sprinkled to help the reader feel what Semmel calls the fierce trident of Britain, its relationship to commerce, and Christianity and Cobdenite conscience. Then, he discusses the waxing and waning of the strategic debate from the end of the Napoleonic wars through the latter half of the nineteenth century, when Britain was losing its economic superiority, began increasingly to feel ill-at-ease in the world, and was forced to make alliances to allow itself to concentrate the fleet in

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home waters against the German menace. By the advent of World War I the debate was between advocates of an offensive close blockade and supporters of the more distant stranglehold that would force the inferior German fleet to sortie and give battle. As the author notes, the three motives behind these debates were economic interest, the prevailing ideology, and power. British policy depended on the chemistry between sociopolitical groupings, Parliament, and the press. In this constant warfare the navy's role has been much overlooked because admirals played a less conspicuous role in Parliament in the nineteenth century than they had in the eighteenth. Only toward the end of Victoria's reign did they begin to have spokesmen who could write, including the American Alfred Thayer Mahan and the doughty Lord Fisher, who vociferously made the navy's case. Through them and the civilians Sir John Laughton and Sir Julian Corbett—as well as because of the greater literacy—naval strategy based on history became a public issue.

Semmel approaches the subject rather differently than a naval historian might. The result, however, is enlightening.

ROBIN HIGHAM
Kansas State University

PAUL B. RICH. *Race and Empire in British Politics.* (Comparative Ethnic and Race Relations Series.) New York: Cambridge University Press. 1986. Pp. xiii, 272. $44.50.

In his introduction Paul B. Rich points out that his purpose in writing the book was to give a historical focus to a subject that had been heretofore largely the preserve of anthropologists and sociologists. He has fulfilled this purpose admirably.

Based on scrupulous and exhaustive research, the book traces British thinking on race and racial differences from the last decade of the nineteenth century to 1960. Rich asserts that British attitudes toward men and women of color in the late nineteenth and early twentieth centuries were strongly influenced by both imperial expansion overseas and industrial growth and conflict at home. What emerged were almost contradictory views of blacks as wild beasts in need of constant scrutiny and control, on the one hand, and of blacks as simple and unspoiled natives in need of protection and the ministrations of missionaries, on the other. E. A. Freeman, the Regius Professor of history at Oxford from 1884 to 1892, was unfortunately not atypical in his views when he exclaimed after a visit to the United States that it would be a simply wonderful country if only every Irishman killed a black and then was hanged for it!

The decline of empire forced a dilution of the most arrogant expressions of British racial superiority and black and Asian inferiority. Although Mary Kingsley was not the originator of the idea, she is still largely credited with the concept of the separate cultural worth and uniqueness of African societies. Kingsley compared the state of African societies in the modern era to that of their European counterparts in the thirteenth century. To her this did not imply the need of carrying the white man's burden in order to lift them up to the level of Europeans but rather the encouragement of a different and separate cultural path. This was the school of cultural relativism. Kingsleyite ideas, of course, came into conflict with more traditional concepts of race relations and empire and thus became adulterated. In time the Commonwealth concept emerged as a compromise between extreme positions of racial segregation, on the one hand, and complete equality, on the other. "The Commonwealth school imbibed some of the teachings of the cultural relativists and extended them on the imperial plane into a set of pragmatic policies that, as far as possible, sought mediation and brokerage between rival schools of colonial policy at the local level" (p. 50).

Rich provides chapters on the widening critique of empire, the role of sociology and anthropology vis-à-vis race, the "half-cast" pathology and its relationship to increased black immigration to Britain after World War II. It is ironic that the concept of colonial development only emerged as the empire was slipping away from Britain, but such was the case. The long period of economic and social neglect came to an end as British colonial administrators, at the eleventh hour, turned to the creation of development schemes to provide for both increased production and the creation of trained colonial political leaders. Rich contends that British colonial planners rationalized the situation by convincing themselves that colonial nationalism was the continuation of imperialism in another guise.

The final chapter of this estimable book is entitled "Empire and the Rise of 'Race Relations,'" and it concerns itself mainly with the changes in British thinking and attitudes prompted by the growth of a significant black and Asian population in the metropole after World War II. For a nation as homogeneous as Britain, accommodation to nonwhite fellow subjects has been neither easy nor peaceful. Government policy and racial liberalism may carry the day, but other voices such as that of Enoch Powell continue to stand in opposition.

ROBERT A. HUTTENBACK
University of California, Santa Barbara

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DAVID HOWELL. *A Lost Left: Three Studies in Socialism and Nationalism*. Chicago: University of Chicago Press. 1986. Pp. viii, 351.

As the subtitle suggests, the thesis of this work is that the issue of the relationship between socialism and nationalism was "central to the politics of the British Left" in the first three decades of the twentieth century (p. 10). To support this argument, David Howell examines the writings and activities of three socialist leaders of the period: James Connolly, the Irish syndicalist who was executed by the British government for his role in the Easter Rebellion, John Maclean, the Scottish Marxist who ended his career isolated from former comrades in both the Communist and Labour parties, and John Wheatley, a cabinet member in the 1924 Labour government who later became an embittered critic of Labour party policy.

Basing his study on the copious writings of these three very articulate socialists, Howell gives a full analysis and critique of the thought of each. It cannot be said, however, that, except in the case of Connolly, the issue of nationalism emerges as central to their theories, let alone to the socialist movement of their time. Maclean, an impeccably orthodox Marxist and internationalist, seized on Scottish nationalism only in the final months of his life as a tactic for justifying the establishment of a Communist party in Scotland that could pursue a strategy more congenial to him than that of the Communist party of Great Britain. Although John Wheatley's proposals for curing unemployment by insulating the British economy might be described as economic nationalism, they were a consequence of his underconsumptionist economic analysis rather than an intrinsic element in his socialism.

By contrast, the relationship between the socialist and nationalist movements was an issue that Connolly in Ireland could scarcely avoid. The political appeal of a Home Rule movement containing bourgeois and rural elements was a potential obstacle to his hopes of establishing socialism through the actions of a united working class. Howell traces Connolly's unsuccessful efforts to develop a viable synthesis of socialist and nationalist politics. In the end nationalism transformed his socialism, for the Protestant workers of Belfast, the most industrialized city in Ireland, were excluded from playing any role in his political strategy.

The Irish situation was, however, unique in Britain. Howell not only fails to prove his central thesis but seems to lose sight of it altogether during lengthy portions of the study. This work remains, however, of considerable value to students of the socialism of the early twentieth century. Connolly, Maclean, and Wheatley were highly intelligent men who devoted their lives to the socialist movement. Their thinking concerning the problems they faced is full of interest. Howell chronicles the development of their thought fully and subjects it to an astute critical analysis.

CATHERINE ANN CLINE
Catholic University of America

PAMELA HORN. *Rural Life in England in the First World War*. New York: St. Martin's or Gill and Macmillan, Dublin. 1984. Pp. 300. $30.00.

Despite the profound changes of the nineteenth century, prior to World War I English agricultural society still displayed many traditional characteristics. Lavish entertainments, shooting parties, and hunt balls were enjoyed by aristocrats. Although they had lost much political power, "within their own rural communities, the nobility and gentry still exercised an undisputed social leadership" (p. 4). Although the use of cash had long been a commonplace, some rural workers were still paid in kind and lived in tied cottages. Rural wages did not keep pace with inflation, there were few alternative occupations, and poverty and poor housing still shaped the lives of laborers.

World War I transformed agrarian society. The drain of adult male laborers into the armed forces combined with the pressure to produce more food, particularly grain, created problems for farmers. The half-century long decline in the use of female labor was reversed, but not without difficulty. The war economy opened up more rewarding and better opportunities for women in factories and transportation, whereas farmers, believing that they could not do the heavier, dirtier jobs, were reluctant to hire them. However, adult women represented a large potential labor pool, and farmers soon grew desperate. Voluntary organizations, notably the Women's National Land Service Corps, were the first to undertake farm work as a patriotic duty, but they could not supply the additional forty thousand full-time female workers needed in 1916. As in so many other cases, only the government had the size and authority to get the job done, and it established the Women's Land Army. Even this could not satisfy the demand for labor. A surprisingly widespread use was made of children. Already disrupted by the absence or death of some family members and the departure of others for nonagrarian employment, children's lives were further changed as they made a significant contribution to the pool of rural labor, often to the detriment of their education. In the last, most difficult, months of the war, "among the most valuable, and perhaps the most unexpected" sources of labor were prisoners of war. Resented by some, accepted by others, the prisoners were a major source of labor and a minor cause of trouble.

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The official response to the critical need for food well illustrates the process by which the government expanded its powers and the range of its intervention. The initial patriotic appeals to farmers to expand arable acreage met with limited success. Later, price controls were introduced, the powers of the Defence of the Realm Act were invoked to determine land use in the national interest, and by 1917 there were demands for the state to play a permanent role in agricultural affairs. Other aspects of this same process well portray the other face of governmental intervention—its often unrealistic and halting approach, its initially slow and clumsy efforts, and its frequent ineffectiveness. For example, it originally believed that patriotism and market forces would provide all the agricultural planning that was needed. Only when the combination of a small, poor harvest and increased submarine attacks in 1916 sharply pushed up prices were price controls finally introduced. Even the flood of later orders and directives, however, could not force all farmers to be efficient or even to keep accurate simple accounts.

This extension of Pamela Horn's already considerable body of work on nineteenth-century agrarian England is a carefully researched and well-written work that strikes a good balance between generalization and detail. Description and analysis are leavened with enough anecdotal material to add interest and humanity. It makes a fine contribution to our understanding of both World War I and the development of twentieth-century British society and government.

JANET ROEBUCK
University of New Mexico

ANTHONY CLAYTON. *The British Empire as a Superpower, 1919–39*. Athens: University of Georgia Press. 1986. Pp. xiv, 545. $30.00.

The intriguing title of this book may not adequately reflect its contents. This is a study of British efforts to maintain power and promote stability throughout the empire—and thus the world—in the interwar period. Anthony Clayton gives some attention to policy but devotes much of the book to describing actions undertaken in pursuit of policy. The work has two sections, one on the 1920s, with emphasis on policing and internal security, the other on the 1930s, when the growing threat of multiple external aggression accelerated the need for rearmament. The organization is identical in each section. There is first a consideration of broad policy, then long and detailed treatment of military commitments in terms of interventions around the world (1920s), responses to aggression (1930s), and the demands of security within the empire. Chapters on military commitments are subdivided into specific historical episodes or geographical areas of empire.

Generally, the work does not break new ground, but it pulls a lot of things together. There is important material here on Britain's changing imperial position. But one must wade through masses of sometimes wearying detail to get to it. Exhaustive data on the composition, equipment, and movement of British forces in various areas, episodes, and campaigns is presented—raising the question of how much one needs to know about the disposition of British troops in the Third Afghan War or the make-up of the Sudanese Camel Corps, for example. The author's ability to range widely over the diverse areas of empire is impressive. So is his knowledge of secondary literature (effectively summarized in a long bibliographical essay) from which the book is basically written. But the absence of footnotes in a work replete with statistics is disconcerting.

We are early told that the British empire was not a military empire; the display of power, with a minimum actual use of force, was the keynote of British imperial policy in the interwar period. This could—and did—tide Britain through numerous internal crises, but the emergence of external threats was another matter. The hope of the 1920s that a time of simple policing lay just ahead drastically changed during the 1930s. British responses to aggression, in particular, lacked authority. Caution, apprehension, and divided counsels became the vogue. As appeasement settled into habit, Britain's enemies came to believe it had lost its nerve—with the result that displays of power progressively ceased to impress. This had repercussions within the empire. Native leaders in India, Palestine, Ireland, and other far-flung areas read the evidence accordingly. Insurgency increased in scope and sophistication and compounded Britain's troubles. In the Middle East, its position remained surprisingly strong, for Arab leaders saw British purposes as more disinterested than those of other powers and most likely to lead to Arab independence.

Clayton observes that Britain's commitments simply outdistanced the nation's capacity to meet them effectively. Even with greater financial wisdom, it is doubtful Britain could have commanded the industrial capacity for rearmament on a scale necessary to preserve its world position. British strategic thinking in the 1930s had some merits, but reliance on a concept of power contributed to Britain's failure to appreciate changing world conditions, especially the way in which disaffected peoples, with little belief in democracy, could be swayed by demagogic mass politics. Power became *machismo* of sorts, and sight was lost of the relationship between resources and the ability to survive, on the one hand, and policy objectives and social values on the other. Yet,

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Britain's "general humanity" and "Christian tradition" left a lasting mark. The empire held together—a fact of decisive importance for the outcome of World War II. These are judgments few will dispute. One is struck throughout, however, by how thinly British resources were stretched, leaving open to question whether the empire should rightfully be seen as a superpower or merely a pretender to such status.

WILLIAM R. ROCK
Bowling Green State University

ROBIN HIGHAM. *Diary of a Disaster: British Aid to Greece, 1940–1941*. Lexington: University of Kentucky Press. 1986. Pp. x, 269. $27.00.

As is to be expected of the author of the *Guide to the Sources of British Military History* (1971), Robin Higham is close to definitive in his long-awaited new book on the British intervention in Greece in 1941. He supplements his study of British official documents at Kew and elsewhere that have been released since 1972 with an effective understanding of the recently opened ULTRA sources as well as his extensive technical knowledge of the Royal Air Force. Higham has not attempted any such thorough survey on the Greek side or that of the Axis, an omission felt most sharply in the relatively brief sections he devotes to actual operations as opposed to planning for the Greek disaster.

Higham's heroes among the Greeks include General Ioannis Metaxas, their shrewd dictator, who, until his most untimely death on the eve of the British decision to intervene in January 1941, wisely resisted British offers of an inadequate number of British infantry on the grounds that this would provoke a German assault. The able Greek commander-in-chief, General Alexander Papagos, likewise is given credit for a truthful account, although the internal Greek political pressures on him not to move his forces from Epirus to Thessaly in time for the German attack are understated.

The "dangerously mercurial" (p. 113) British foreign minister, Anthony Eden, emerges as the real culprit in Higham's strictures, both in his politically motivated actions in behalf of intervention and in savage descriptions of his publicity-conscious preening and overriding of his more realistic military advisors. After his previous fiasco with ANZAC troops in the Aegean in 1915, the more careful and experienced Winston Churchill somewhat escaped his full responsibility for Greece, notwithstanding his passion for dispersing Britain's limited military resources at the expense of the hitherto successful and far more important British drive from Egypt on Tripoli.

The much-tried British commander-in-chief in the Middle East, General Sir Archibald Wavell, is clearly one of Higham's heroes, although, like the equally exhausted chief of imperial staff, Sir John Dill, he dithered a good deal rather than openly opposing his political superiors on Greece. Air Chief Marshall Sir Arthur Longmore, in charge of the Royal Air Force in Greece, is justly portrayed as another scapegoat for the hopeless lack of up-to-date equipment of all types in the Middle East in 1941. Higham is particularly good at clarifying Longmore's logistic nightmares both in Greece and on the clogged air-ferry route across central Africa to Cairo.

I especially appreciated Higham on the unfortunate influence of the Hellenophile British ambassador in Athens, Sir Michael Palairet, who, like his political masters Churchill and Eden, so often appears to have lived in the era of Palmerston's Crimean War, when Great Britain could still afford such chivalric, useless, and expensive gestures, given its then overwhelming fleet and the absence of hostile airpower. Higham is also not fooled by the "Irish blarney" (p. 134) of that itinerant meddler, Colonel William Donovan of subsequent O.S.S. fame, but Donovan's still more disastrous influence on the belated Yugoslav resistance to Hitler is not mentioned. Churchill's exploitation of another vastly inflated luminary, Premier Jan Christian Smuts of South Africa, as a cover for his own decision, is touched on briefly, but the continual protests of Australian Premier Robert Menzies, the only man with the courage to stand up to Churchill in the War Cabinet in London, are given full credit.

What appears most striking about Higham's remarkable portrait gallery is the sheer incompetence of the higher direction of war in Britain so late as 1941, an incompetence revealed again in Eden's pathetic invasion of Suez in 1956. It is to be hoped that the devastating weakness in radio communications at all levels in 1941 has at last been remedied today, as the finely run campaign in the Falklands would seem to imply. In any event, unnecessary interventions on a shoestring, in which the reluctant military simultaneously have to plan for evacuation, will no doubt continue to plague us all.

TRUMBULL HIGGINS
City University of New York

STEVEN G. ELLIS. *Tudor Ireland: Crown, Community, and the Conflict of Cultures, 1470–1603*. New York: Longman. 1985. Pp. x, 388. $16.95.

All students of the history of Britain and Ireland will welcome this learned and readable synthesis of scholarship on the subject. Viewpoints apart, all will also recognize the work of a sober, industrious

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master. In a sense, Steven C. Ellis does for Ireland what G. R. Elton has done for Tudor England.

The volume is admirably produced, with plentiful footnotes, maps, tables, and two bibliographies, one (entitled "Guide to Further Reading") a critical one. It does not pretend to represent new research, but the list of the author's publications shows that he has done his fair share. To this neophyte it would seem that he has the advantage over his colleagues in English history of a relative dearth of good surveys.

Ellis's standpoint is that of the Tudor rulers. He does not seem to depart radically from accepted interpretations. His textual and footnote references are most often informative rather than critical. He also does not neglect the embattled Celticists. Two points I noticed were the rejection of an "Irish nationalist" interpretation and an insistence on continuity from medieval into modern Irish history.

To many readers, the findings on religious and church matters will be a bit surprising. Reflecting, as always, modern research, Ellis shows that, with certain exceptions, neither ultramontane nor Protestant forces played a major role. Skilled, accommodating leaders on both sides prevented any "final solutions." This area is due for extensive revision in any case. Probably, Ellis's account errs on the side of underestimating the force of beliefs at work on both sides. The reported historiography certainly suggests an undue prevalence of Gaelic, medieval enquiry in the groves of academe. Perhaps church historians will take the hint.

At the opposite end of the spectrum, that is, in the treatment of economic matters, Ellis is a mere summarizer. This simply will not do. Even with the excuse of wretched writing on the subject, I would side with those who wish to see material conditions and nonpolitical groups properly woven into the story of the development of any national community.

An example of two of these generalized comments would help the reader judge the author's achievement more fairly. First, his reflection of consensus is shown in his account of the Battle of the Yellow Ford in 1598, which is firmly based on the work of G. A. Hayes-McCoy and Cyril Falls. The battle's importance, too, as a major turning point remains undisturbed. Also undisturbed are the letters, papers, and reports on which revision could be based.

Ellis's treatment of Elizabethan policy, again, does not overthrow previous opinion. But it reveals a far more complex range of features than those found in previous surveys. Of these, the extension of judicial powers as a necessary administrative, rather than as a sinister imperialist, program is notable. So, too, the role of extremist adventurers on both sides is contrasted with the generally politic approaches of the major leaders. In these revisions and qualifications the new views are based on Ellis's own specialized writings, in the first instance, and on those of, for example, C. Brady and B. Bradshaw, in the second.

M. R. POWICKE
University of Toronto

JOHN P. DUGGAN. *Neutral Ireland and the Third Reich.* Totowa, N.J.: Barnes and Noble or Gill and Macmillan, Dublin. 1985. Pp. xxiii, 295. $31.50.

In undertaking to cast new light on Ireland's neutrality in World War II, John P. Duggan has used newly released archival sources and interviewed many of the Irish, British, and German principals. He concludes that, although neutrality was a workable policy for Ireland then, in a modern technological war not even an Ireland blessed with diplomats as clever as Eamon de Valera could sustain such a policy.

Duggan brings a military perspective to the place of Ireland in a possible German invasion of England. He maintains that an invasion would have split the British forces and that Winston Churchill was concerned enough about the potential danger to consider drenching Ireland with poison gas should this come to pass.

Some of the figures of the era, such as Charles Bewley, the pro-Nazi Irish minister to Germany in the 1930s, are provocatively described. After leaving the diplomatic service, Bewley devoted himself to writing "waspish reports and poisonous little notes" about his colleagues, alleging that the Foreign Office was inefficient because Prime Minister de Valera was too miserly to hire extra typists. De Valera, the architect of Irish neutrality, is not portrayed clearly. We see him through the eyes of his contemporaries, extreme republicans who thought him too pro-British and the Anglophiles who felt him not sympathetic enough. Certainly, de Valera was an ambiguous man, who "never mishandled words," a revolutionary who had become a constitutionalist. Berlin never fully understood what a small role he was willing to play on their behalf.

The book centers on Adolf Hitler's minister to Ireland from 1937 to 1945, Eduard Hempel, who stands in sharp contrast to de Valera and was occasionally mesmerized by him. Hempel has often been described as a "diplomat of the old school," and other writers have claimed he was not a Nazi. Duggan says this is making apologies for a man who served Hitler's regime until its demise and who was favored by the Ausland organization in Ireland.

Until 1939 Hempel kept Berlin informed mainly on emerging Irish neutrality and partition, and he suggested that the Irish Republican Army (IRA) might be pro-German. Although aware of American influence on Irish affairs, he did not understand

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the complexities of American politics. He believed that Franklin D. Roosevelt's reelection in 1940 would lead to increased pressure on Ireland to abandon neutrality and thus overlooked the benefits that the Irish position provided the Allies. Duggan finds Hempel a cautious thinker who was probably appropriate for the job. He asserts that the German legation transmitted information about the proposed airborne landings at Arnhem and the August 1942 raid on Dieppe. The former news came to Hempel via an Irishman working in a British aircraft factory. The latter may have been procured by Hempel's second in command, Henning Thomsen, an avid Nazi who actively sought information, especially in barrooms.

Although such anecdotes pique the reader's interest, the book is ambivalent, as though the author could not decide whether to write an analysis of diplomacy, a spy story, or a political yarn. Ultimately, Duggan replows old ground and neglects to expand on his newer information. He leaves us hanging about the fascinating characters who played out this drama, therefore, few of the diplomats and none of the German agents really "live." He is at his best describing those he dislikes, for example Bewley and General Eoin O'Duffy. There are no maps, pictures, or scholarly citations. Footnotes tend to be "fillers." Important people are not fully identified, first names are omitted, and far too much is made of Eva Hempel as a source. Although her personal recollections about people within the legation are interesting, her comments on diplomacy are irrelevant, petty, defensive, and self-serving. There are errors, too. The statement that Hempel was favorably inclined toward involvement with the IRA is not borne out by documentation. Indeed, even the focus on Hempel is dubious, since he and his legation were only part of the total picture of Ireland in the war. Too bad the author did not try to explain why the Germans were misled by Irish rhetoric regarding Irish-British relations. Was it because the Germans did not understand any kind of nationalism except their own?

CAROLLE J. CARTER
Menlo College

MACK P. HOLT. *The Duke of Anjou and the Politique Struggle during the Wars of Religion*. (Cambridge Studies in Early Modern History.) New York: Cambridge University Press. 1986. Pp. xiii, 242. $34.50.

In 1576 Thomas Wilson, Queen Elizabeth I's agent in the Netherlands, wrote that the fate of Christendom rested in the hands of three men: Don John of Austria, William of Orange, and François de Valois, duke of Anjou. Even allowing for Wilson's hyperbole, it is indeed curious, as Mack P. Holt points out in the introduction to this excellent study of the duke of Anjou (1555–84), that in four centuries of historical writing about the French Wars of Religion not one serious or popular biography or monograph has been devoted to Anjou's life and career. "Because all his grandiose dreams ended in frustration and his career was cut short by a premature death, Anjou has been assigned merely a walk-on role on the European stage . . . This is hardly a just reward for someone who was considered one of the three most important men in all Christendom" (p. 1). What historians have seemed to forget, although his contemporaries never could nor did, was that Anjou was destined to become the next king of France—a Francis III—once death would remove his childless brother Henry III from the throne.

Anjou's importance, however, was linked as much to his actions as to anticipation of his future royal status. He was involved in consequential ways in the Wars of Religion—in rebellion against the French crown as well as in royal attempts to crush the Huguenots, in more than merely formal and hopeless negotiations for the hand of Elizabeth I, and as the titular sovereign of the United Provinces. Yet all his schemes came to naught, and he died a failure, excoriated by many and praised by few, leaving behind a career that, as any student of the period can attest, is as confusing in its complexity and as baffling in its significance as that of any of the prominent historical actors of the second half of the sixteenth century.

Holt has now splendidly filled this historiographical gap. He expertly traces and examines Anjou's life and career based on work in French, English, Dutch, and Belgian archives, as well as German, Spanish, and Italian sources. The importance of a person such as Anjou and the role he played are critical to any understanding of French politics, domestic affairs, and international relations during the period. Holt's book illuminates and analyzes this on a number of levels. He examines Anjou's relations with the royal family, the greater nobility, the Huguenots, and the Dutch with equal facility. The method is traditional narrative history—the type produced for this period by such scholars as N. M. Sutherland, Nancy Roelker, De Lamar Jensen, H. G. Koenigsberger, J. H. Elliott, and Geoffrey Parker. But he also deals with socioeconomic themes, such as the finances of Anjou's appanage and household as well as the sources and amount of funds he had available to mount his various military operations.

A sympathetic but sober reassessment of Anjou, the book also explores the more general issue, acute for the early modern period, of the difficulties and dangers involved in any great subject developing an autonomous base of power and influence commen-

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surate with his personal status and the problems and threat this posed for the development and maintenance of state power and authority. In Anjou's case this struggle was only solved for France by the dangerous expedient of allowing him to cast his net into troubled international waters, although Holt maintains that in both France and the Low Countries "the duke of Anjou's greatest failure was his unwillingness to become part of the *politique* struggle" (p. 214), which may be to anticipate a constellation of factors that are in part hard to envision coming into existence while he was still alive. In the end, the most important thing Anjou did was to die, which drew France into another round of struggle (the War of the Three Henrys) and ultimately enabled Henry IV both to desert the Huguenots and to institute temporary tolerance of them as *politique* king of Catholic France.

The life of the much maligned younger brother of Henry III is a most complex and confusing subject. I confess that despite all the reading I have done on the period I never quite understood Anjou's role. Holt so clearly and expertly traces and explains his career and the role he played in a number of different contexts that after finishing the book I felt that for the first time I understood not only Anjou but also the significance of Anjou (which was often quite separate from his person) as a great subject, heir to the throne, international bargaining chip, and meddlesome, bumbling, and ultimately tragically unsuccessful adventurer. This most carefully researched and crafted book will be the definitive work on its subject. It is an excellent contribution to the study of the early and middle period of the Wars of Religion and should be of interest to scholars of Tudor England and the Low Countries as well as Europe.

JAMES B. WOOD
Williams College

FREDERIC J. BAUMGARTNER. *Change and Continuity in the French Episcopate: The Bishops and the Wars of Religion, 1547–1610.* (Duke Monographs in Medieval and Renaissance Studies, number 7.) Durham: Duke University Press. 1986. Pp. xiv, 324. $30.00.

There are two parts to this book. The title refers mainly to chapters 10 and 11. The bulk of the book brings together the work of others to provide a comprehensive, unflattering picture of the bishops of sixteenth-century France.

Topics that are particularly well treated are the Concordat of 1516, the appointment policies of the kings, the role of nobles in episcopal appointments, residency, types of benefices, pluralism, and the succession of Henry IV. Much more could have been made of both synodal and pastoral visit records, even if only the printed *répertoires* were used. More comparative work could have been done with episcopal family relationships, education, and office-holding patterns.

Frederic J. Baumgartner accurately presents the bishops of sixteenth-century France as men who held a number of benefices simultaneously, often paid little attention to their religious duties, frequently did not reside in their dioceses, and were slow to react to Protestantism, even though it was a threat to their existence. A clearer presentation of the viewpoint of the bishops themselves, concerning their roles as governmental and ecclesiastical officials who had to be away from their dioceses, would have been useful. It is also possible that Baumgartner is insufficiently sympathetic to bishops who favored humanism or humanists and those who were *politiques*.

As for the material covered by the title of the book, the author fills in an important gap in our knowledge of the social origins of French bishops. The statistics for the period 1547–94 are now known. Unfortunately, Baumgartner becomes too involved in trying to prove that he also has a new, exclusive explanation for the episcopal appointment activities of Henry IV between 1589 and 1610. This explanation, that Henry at first rewarded his followers and then reverted to older patterns of appointment, may have validity (if further work provides concrete support), or it may be a version of the older theory that Henry IV was trying to win support from all groups in society. The views that Henry IV was looking for reformers and that he chose bishops from the same group from which he was choosing government officials also must be considered.

Thanks to Baumgartner, we now know that Henry IV appointed bishops from the Third Estate at a rate similar to that of Charles IX and Henry III. Despite Baumgartner, it remains true that Henry IV appointed significantly more members of the new nobility to the episcopate than had earlier monarchs and that these bishops, at least in holding synods, were more active reformers than other bishops. Also ignored is the fact that these bishops (including Richelieu) were around for a long time and had a profound influence on France. What is needed now is a study of the period between 1614 and 1681.

As for Henry IV's motives, who knows? As with his foreign policy, Henry never said clearly what he was doing, and there is no way for historians to know for sure.

J. MICHAEL HAYDEN
University of Saskatchewan

BAILEY STONE. *The French Parlements and the Crisis of the Old Regime.* Chapel Hill: University of North Carolina Press. 1986. Pp. x, 326. $35.00.

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In his latest book Bailey Stone has extended the scope of his previous work on the Parlement of Paris to include all fifteen parlements of the Bourbon monarchy in the eighteenth century. As in his earlier book, Stone examines two propositions or themes: that the two thousand parlementary magistrates did *not* speak for the French nobility as a whole or lead an "aristocratic revolution" that would culminate in the convocation of the Estates-General in 1789 and that the role of the parlements should be seen as part of an increasingly intense constitutional debate in which the parlementaires attempted to guide the diverse elements of resistance to "ministerial despotism," while maintaining their traditional function as defenders of absolute monarchy, social hierarchy, corporative prerogative, and personal privilege.

That the remonstrances of the magistrates cannot be interpreted as a mechanical reflex to class interests seems beyond dispute. That the magistrates frequently defended the interests of social groups other than their own is also well established. Steven Kaplan (*Bread, Politics, and Political Economy in the Reign of Louis XV*, [1976]) conclusively demonstrates that the parlements often defended the bread consumer from the disastrous effects of free trade in grain. But to claim that the robe nobles "were assuredly not altogether wanting as sincere and informed spokesmen for underprivileged France" is perhaps to go too far (p. 251). In general Stone is very careful not to exaggerate the "paternalist humanitarianism" of an overconfident judicial establishment. In both of his studies of the parlements he wisely stresses the robe's ambivalence on almost every issue, from the commutation of the *corvée* to the reduction of the national deficit.

Although Stone's wider canvas does not yield any general conclusions that differ substantially from his earlier ones concerning the Parlement of Paris, he has now given them a nationwide institutional basis. Moreover, Stone has labored through the prodigious secondary literature on the provincial parlements and the robe nobility as well as the remonstrances, judicial rulings, opinions, and correspondence found in local archives from Rennes to Toulouse, from Dijon to Aix-en-Provence. He has deployed his findings with clarity and economy, demonstrating the commonalities of opinion among all the parlements, despite personal and philosophical rivalries, the frequent reversal of decisions, the eclecticism in the choice of precedents and vocabulary, and rhetorical excess, even by the tolerant standards of the century.

My principal criticism of this book is that it does not attempt to relate the social and economic role of the magistrates, especially as landlords and seigneurs, to their opinions on the bench. One does not need to subscribe to any particular interpretation of the origins of the French revolution to notice the consistent defense of the *seigneurie* by all parlements. In his effort to establish the magistrate as a constitutional legist, Stone may have underplayed the magistrate's role as a seigneur who knew every treatise on feudal law, indeed even wrote many of them. If the seigneurial regime seemed harsher in the last half of the eighteenth century, and if the peasant revolution was more than just another *jacquerie*, then the magistrate as seigneur as well as judge must bear a major share of the responsibility.

ROBERT FORSTER
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BRIAN C. J. SINGER. *Society, Theory, and the French Revolution: Studies in the Revolutionary Imaginary*. New York: St. Martin's. 1986. Pp. 236.

This is a difficult and dense book with an important central insight: that society only became visible (that is, an object that could be acted on) during the French revolution. According to Brian C. J. Singer, the religious "imaginary" suppressed the discovery of society until secularization made possible a new view of representation. A critical first step was taken when the Enlightenment provided a critique of religion that could engender a science of society. In the revolution society was presented as an entity that could be fundamentally transformed by conscious action. Society then became conscious of its institution, and ideology appeared for the first time. Ideology was impossible without this kind of social "reflexivity." In the process of developing increasing social visibility, the representation of power under absolutism lost its character as the representation of divine and immutable truth and appeared as a theatrical spectacle filled with artifice. Power came to be conceptualized as immanent to society rather than as an expression of the ruler's power over society; society was seen as constituting the external power of the state by itself.

As can be seen from this bare-bones summary, the book is filled with abstractions that are sometimes hard to tie to events or to authors, books, or tracts. Only three revolutionary writers are examined in any detail: Volney, Sieyès, and Saint-Just. Even in these chapters the writing is sometimes convoluted (an impression that is not helped by the distressing number of typographical errors). The difficulty follows in part from the aim that the author has set himself, which is the writing of a philosophical history of the emergence of new conceptions of society and politics ("a history of social visibility" [p. 5]) rather than an intellectual or political history properly speaking. There is consequently no survey of revolutionary thought on any of the major topics

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considered; instead, a central thread of argument is pursued using presumably exemplary works when useful and ignoring the rest.

The most valuable of the chapters is the one on power and constitution, because it includes an excellent discussion of the principles underlying constitutional thought (in Sieyès). Here the virtues of the author's method are most apparent. He is able to dig beneath the superficial disagreements expressed in publications of the revolutionary decade and examine central principles that proved both fruitful and problematic. He argues in this chapter, for example, that the revolution could not be made to promote the free market economy, because the kind of social and conceptual change involved in the revolutionary process necessarily focused attention on political concerns. The final chapter on the Terror, in contrast, is not very original or decisive. Although the author wants to mark himself off from those who are concerned with the origins of totalitarian democracy, he himself traces almost everything back to philosophical problems in representation. The afterword sounds much like Hannah Arendt with its references to the social question as opposed to the political question. The basic affiliation of the book, however, is more in line with the work of Cornelius Castoriadis, Claude Lefort, and Maurcel Gauchet, who deserve to be better known among historians.

LYNN HUNT
University of Pennsylvania

STEVEN LAURENCE KAPLAN and CYNTHIA J. KOEPP, editors. *Work in France: Representations, Training, Organization, and Practice*. Ithaca: Cornell University Press. 1986. Pp. 576. Cloth $22.50, paper $7.95.

This hefty volume of seventeen essays originally presented to a conference on the representations of work in France offers applications of cultural anthropology to the study of work and workers from eighteenth-century *compagnonnages* through twentieth-century autoworkers. Representations and meanings receive more attention than the organization and practice mentioned in the subtitle. Power relationships are of more interest than impersonal economic forces; segmentation of the labor force is emphasized over class solidarity; and individualistic protests against working conditions predominate over collective actions. Work, as a physical process, and the workplace, as a physical place, are almost absent.

Seven essays treat the politics of work relations. Five deal with hegemonic conceptions of work; for instance, Steven Laurence Kaplan shows how guild masters saw the social distinctions of the guild structure as tools to control journeymen, and Cynthia J. Koepp reveals how the *Encyclopedie* tried to "appropriate" workers' language to facilitate bourgeois management of labor. William Sewell's essay explores neglected sources for the history of labor relations, visual images. Others critique familiar sources, as in Joan Scott's demonstration of the political purposes of the *Statistique de l'industrie à Paris*.

Four essays challenge received wisdom about artisans. Some use case studies but stretch their evidence. Rejecting Albert Souboul's view of workplace intimacy, Michael Sonenscher argues that a "fiction of intimacy" covered a reality of instability in workshop life. He makes a promising but unproven suggestion that affinities developed in the *logis* facilitated recruitment of a mobile labor force and collective action.

Other essays raise the issue of fragmentation of the working class. A thought-provoking essay entitled "The Myth of the Artisan" by Jacques Ranciere questions the connection between skill and militancy and contends that assertions by the avant-garde should not be interpreted as the collective voice of all workers. More generally, he faults cultural anthropology for assuming cultural homogeneity. Yet other essayists accept the segmentation of the labor market. Although Michael Hanagan stresses that family issues could reconcile capitalists with workers, he also notes that mining companies could divide single from married men.

Only three essays interpret workers' representations and find relatively little directly related to their work. In a nineteenth-century autobiography Michele Perrot finds an ambivalent worker who labored as a means of existence yet thought work was a reward for virtue, a man who did not like the discipline of work schedules yet appreciated the cleanliness and community in the mill. William Reddy's essay on the patois literature of Lille factory laborers goes further in understanding the virtual silence on the actual work and the importance of farce as a protest against degradation.

In reflective pieces Maurice Garden and Christopher Johnson note that this collection indicates the need for more attention to the family, the community, and the conditions of life, as opposed to work itself. However, the reader may also wish for more on the actual practice of work: what were the tasks, who did which task, and how did they do it?

MARY LYNN STEWART
Simon Fraser University

J. P. T. BURY and R. P. TOMBS. *Thiers, 1797–1877: A Political Life*. Boston: Allen and Unwin. 1986. Pp. xi, 307. $39.95.

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J. P. T. Bury and R. P. Tombs tell us that this is the first scholarly biography of Louis-Adolphe Thiers. The question arises, why has it been so long in coming? Could it be that Thiers does not merit a biography? He is, after all, not a very interesting man. He was vain, wrong-headed, and nonreflective; he was callous and mean. Even the histories he wrote, which are his chief claim to intellectual standing, are unread today and probably deservedly so. Despite the torrent of words that came from the man, one suspects that they do not reveal much more than the mind and spirit of a pedestrian person, too pedestrian to warrant full-scale biography. The authors note, however, that there are "few works on French political and social history in the nineteenth century in which the name Thiers does not occur" (p. ix). If not his personality, then, should not his political prominence have justified biography? The answer has apparently been no. Thiers was not a modern man. Everybody knows that he did not appreciate the economic significance of the railroads. More important, he did not seem to realize that France was a declining world power and what was required to reverse the trend. Although it would be excessive to blame Thiers for the decline, his policies certainly did not work to prevent it. For those interested in the political, economic, and social problems that France has faced in the twentieth century, Thiers's career is mostly irrelevant.

The fact that this biography has appeared does not gainsay these points. Nothing in the book is psychoanalytical. No attempt is made to delve very deeply into the inner man. Unless the authors felt ill-prepared to take up such a psychoanalytical task, they must have thought that the personal and intellectual life of Thiers did not merit such an approach. Nor does the book attempt to deal with Thiers's political life in twentieth-century terms. Hints are given about Thiers's failure to prepare his country for its difficult future, but the historical roots of twentieth-century France are not the book's chief focus. Rather, it is written for people who are interested in nineteenth-century French politics, by people who know the political events well. This means that the authors (in thirteen chapters, which cover the Restoration, the July Monarchy, the Second Republic, the Second Empire, and the beginning of the Third Republic) describe Thiers's role in France's effort to find a stable political system. The story, then, is less a biography than a general French history in which Thiers's political life is enmeshed. There are no thundering revelations along the way, for the book deals with familiar territory. There is little doubt, too, where the authors' sympathies lie; in a world in which politics is the art of the possible, Thiers's solution to the problem, the conservative republic, emerges as the best result for France. The book is a competent political study meant for nineteenth-century specialists. It looks back to the legacy of the revolution and the empire, not forward into our present. It is in its method and style almost of the nineteenth century itself. For those who are tired of the tyranny of the present over the past, the volume might offer quiet relief.

ROBERT R. LOCKE
University of Hawaii

AUSTIN GOUGH. *Paris and Rome: The Gallican Church and the Ultramontane Campaign, 1848–1853*. New York: Clarendon Press of Oxford University Press. 1986. Pp. xvi, 276. $49.95.

Austin Gough has written a valuable monograph. The main thesis of the book—that Gallicanism in the French church was mortally wounded by 1853 as a result of the rising tide of a new ultramontanism and the publication of the papal encyclical *Inter multiplices*—is not new. It originated with Gallicans of the day and has been reiterated by historians ever since. But Gough brings together a story that we knew only in a fragmentary way before and tells it in much greater detail.

The book is well written, and Gough gives a balanced and fully nuanced historical picture. The first five chapters provide background; the last eight narrate developments after 1848, substantiating fully that the years 1848 to 1853 were decisive for the triumph of French ultramontanism.

In mid-nineteenth-century France, the conflict between ultramontanism and Gallicanism was, "in essence, a conflict between the belief that the challenges of post-Revolutionary society could be met only by a centralized Church, uniform in doctrine, style, and discipline, controlled by an infallible Pope and a vigilant Roman administration, and the contrary belief that the Church would lose all influence in modern society unless it had deep roots in national character and local institutions, and a looser, federal, collegial structure" (pp. vi–vii). Gough proffers a comprehensive view of this conflict, but Archbishop Auguste Dominique Sibour of Paris and Bishop Félix Dupanloup of Orleans are the main figures for the Gallicans, and Louis Veuillot, Dom Prosper Guéranger, and the apostolic nuncios, R. Fornari and Antonio Garibaldi, are the main figures for the ultramontanes. The Catholic press is also terribly important; the evidence even suggests it was decisive. Gough shows how *L'Univers* worked closely with *Le Correspondance de Rome* and *Civilità cattolica* to incite clerical indiscipline in dioceses with Gallican bishops, intimidate Gallicans with threats of heresy, and leak information from high church sources damaging to the Gallicans. The book provides good analyses of the main issues around which the conflict evolved: the authority of bishops in their

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dioceses, the power of provincial councils vis-à-vis Rome, the role of the Catholic press in church politics and dogma, the introduction of the Roman rite into French dioceses, and the choice of seminary textbooks. And we are shown how the evolving political situation and the growth of Voltaireanism and secularism fueled the conflict.

Gough hypothesizes that Gallicans could have crippled the ultramontane assault had they ignored the nuncio's interpretation of canon law and formed a united front. He believes the contest was still open in 1851 and 1852. But Sibour made unwise protests to Rome on at least two occasions, leading to a further crystallization of ultramontanism, and by April 1853, with the publication of *Inter multiplices* condemning Gallicanism, the debate was closed.

Had I refereed the volume for Clarendon Press, I would have recommended three additions: a conclusion bringing together the most important subtheses in the book and providing a fully nuanced statement of the main thesis, a section in the preface locating the study within the context of preexisting scholarship, and a bibliography.

SANDRA HORVATH-PETERSON
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EUGEN WEBER. *France: Fin de Siècle*. Cambridge: Belknap Press of Harvard University Press. 1986. Pp. x, 294. $20.00.

This is a book to delight nostalgia buffs as well as to enlighten serious historians of modern France. It is representative Weber: a highly personal, impressionistic journey through the last two decades of the nineteenth century, written with flair and style. Its main focus is on how the French lived and how they viewed themselves and their society in the era just before the Belle Epoque. The work rests on a lifetime of immersion in the archives and libraries of France and on the author's unusual sensibility for all things French.

Historians, like lovers, are inclined to believe that the object of their interest is in some way unique. Eugen Weber disclaims any such belief about his subject. "What makes the fin de siècle interesting," he asserts, "is its unexceptionable nature: the way it reflects the nineteenth century to which it belongs; the way it announces the twentieth century already taking shape; the continuities it affirms in the midst of change; the changes it experiences amid persistent continuity" (p. 235). Yet his book begins with a suggestion that the period was indeed exceptional in at least one respect: it displayed a startling "discrepancy between material progress and spiritual dejection," and thus, he believes, resembles our own times (p. 2). These contradictory aspects of the *fin de siècle* are abundantly illustrated by piquant examples. The initial chapter, entitled "Decadence?" sets the tone. The theme is not new, but Weber's evidence demonstrates a kind of neurotic obsession with the subject in intellectual and literary circles. Some of the French viewed the signs of decadence with deep concern and sought to stop the rot by some kind of national renewal; others found it exciting, a corrective to mediocrity, and turned it into a cult.

Subsequent chapters range widely through French life and mores: the stresses of intrafamily relationships, the prevalence of envy and suspicion (surely not confined to the *fin de siècle*!), the new passion for cycling, the arrival of organized sport, the development of tourist resorts. Now and again, Weber's taste for the bizarre may lead some readers to assume that deviant behavior (crime, kinky sex, wife selling, the premature dispatching of old people by covetous heirs) came to be something like the norm in those days. But it would be unjust to seek quibbles with an author who continues to give us books that are so perceptive, so readable, so brilliantly conceived. No wonder that Weber fans await each one with anticipation and are never disappointed.

GORDON WRIGHT
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DÁMASO DE LARIO. *El comte-duc d'Olivares i el Regne de València* [The Count-Duke of Olivares and the Kingdom of Valencia]. (La Unitat, number 97.) Valencia: Eliseu Climent. 1986. Pp. 369.

One of the more encouraging tendencies within recent studies of early modern Spanish politics has been a revival of interest in constitutional history. A growing number of scholars, including Pablo Fernández Albadalejo, Xavier Gil, Charles Jago, and I. A. A. Thompson, not only have challenged received wisdom concerning the docility of Castilian parliaments but also have begun to question traditional interpretations of the constitutional development of the more assertive *foral* regimes in the rest of the Iberian peninsula and Italy. It is within this revisionist context that one welcomes the publication of Dámaso de Lario's study of the Valencian *corts* of 1626, a crucial year for the Spanish monarchy. In a concerted effort to enhance royal authority in the kingdoms outside Castile—a design that promised significant increases in the revenues of a government hard-pressed to finance its heavy commitments abroad—Philip IV convoked simultaneous meetings of the parliaments of Aragon, Catalonia, and Valencia. The royal favorite Olivares unveiled before the assembled deputies his "Union of Arms," an ambitious plan for closer military, political, and financial cooperation among the dif-

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ferent realms of the monarchy. The first half of this book provides a day-to-day account of the protracted negotiations between the crown and the three estates (clergy, nobility, and commons) of the kingdom of Valencia. Despite a pronounced lack of enthusiasm for Olivares's project, the Valencians proved far easier to tame than the Catalans. The latter's intransigent defense of established parliamentary procedures—in marked contrast with the "weakness" of Valencian and Aragonese constitutionalism—contributed to widening the gap between the principality and Olivares's regime, which ultimately led to the Catalan revolt of 1640.

The second half of de Lario's study consists of a detailed analysis of the laws passed during the parliamentary session. This body of legislation provides interesting glimpses into numerous aspects of seventeenth-century Valencian society and government, ranging from corruption in the law courts and fraudulent banking practices to attempts to ease restrictions on dowries, reform the selection of aldermen, and expand the jurisdiction of seigneurial tribunals. That the great majority of these proposals clearly favored the interests of the privileged classes combined with reports of open bribery and the purchase of votes within the estates to give rise to widespread popular criticism. Protests included pasquinades lampooning the royal favorite and the parliamentary deputies, along with more private laments, such as the impassioned diary entry of the local cleric who prayed for divine punishment of those "traitors . . . to their fatherland and their nation's privileges, who by selling out the poor have reaped great profits for themselves." Despite their bitterness, these complaints were to no avail. Olivares's skillful combination of blandishments and bullying converted the parliament of 1626 into one of his more important domestic political victories and paved the way for the close collaboration between Valencia's ruling class and the central government that marked the rest of the century.

This book provides a well-written and thoroughly documented account of a significant episode in the relations between the Spanish crown and one of its realms during the troubled reign of Philip IV. As such it will serve as a useful supplement to John Elliott's definitive narrative of the Olivares regime and to future efforts to chart the still unfamiliar waters of Spanish parliamentary history.

JAMES S. AMELANG
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CARLA RAHN PHILLIPS. *Six Galleons for the King of Spain: Imperial Defense in the Early Seventeenth Century.* Baltimore: Johns Hopkins University Press. 1986. Pp. xiv, 318. $37.50.

Carla Rahn Phillips explains in her preface that, using as a focus six galleons built for the Castilian crown in 1625–28, she discusses three sets of topics in the lives of armada ships. The first involves their construction, including the costs and availability of labor and raw materials on the north coast of Spain where they were built. To place the six galleons in context, she examines the background of ship construction in Spain and the effects of inflation and royal policies on the shipbuilding industry in the sixteenth and seventeenth centuries. The second set of topics focuses on the preparation of the galleons for the Indies run (*carrera*), an impressive feat of logistics. Her discussion of where the food and equipment came from, what they cost, and how difficult they were to procure certainly contributes, as she claims, to a better understanding of the bureaucratic responses to the challenge of defending the empire. Third, she examines the lives of the men and the ships that composed Spain's imperial fleets, concluding with a sketch of the six galleons, from their first voyage to the Indies in 1629 to after the Battle of the Downs in October 1640. It can be said at once that she has admirably fulfilled all her objectives and produced a most satisfying and innovative work.

The bedrock of her research is a packet of inventories in the James Ford Bell Library at the University of Minnesota, describing the six ships delivered to the crown. They give a fairly detailed inventory of the equipment and characteristics of each ship, all of them built by Martín de Arana of Bilbao under a contract with the crown signed at Madrid on March 14, 1625. Through her research in the Spanish archives, Phillips has traced the progress and vicissitudes of this contract and the career of each of the galleons. Among the many interesting points that emerge are how the royal bureaucracy oversaw the work without unduly interfering with it and the insistence of the crown that the men on board should be adequately fed. Her analysis of the shipboard diet shows that it contained plentiful calories and adequate nutritional content, by both seventeenth- and twentieth-century standards. Chapter 6, "Officers and Men," has an excellent discussion of the Iberian fixation that the soldier's vocation was vastly superior to that of the seaman. Crown legislation tried sometimes to rectify this anomaly but with no lasting results.

Grand strategic plans for impressive fleets were rarely matched by a willingness of the crown to spend the money needed to implement them, which was apt to be diverted to prestigious court projects such as the Palace of Buen Retiro. Philip IV and his advisers often demanded more than the fleets and their best commanders could possibly achieve. Nevertheless, the commanders managed in spite of everything to keep the *carrera* going, as exemplified

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by its speedy recovery after the capture of the Mexican silver fleet by Piet Heyn at Matanzas in 1628.

There are, inevitably, a few factual errors. On page 99 for Calcutta read Calicut. On page 209 it is erroneously stated that Don Lope de Hoces recaptured Curação from the Dutch in 1636. On page 219 the Dutch losses at the Battle of the Downs are greatly exaggerated. They only lost one warship (not ten) and one hundred men (not one thousand). But these and a very few other trifles do not detract from an excellent work that is required reading for all serious students of seventeenth-century maritime history.

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ANDREAS DORPALEN. *German History in Marxist Perspective: The East German Approach.* Detroit: Wayne State University Press. 1985. Pp. 542. $45.00.

This massive book by Andreas Dorpalen surveys historical writing in the German Democratic Republic (GDR) from 1949 to 1978. Dorpalen introduces the book with a chapter on the principles of historical materialism (which he refers to as "Marxism-Leninism" and "Marxism") and the institutional frameworks—state, party, university, and academy—of historical writing in the GDR. Eight chapters then mark the passage, period by period, through German history from the Middle Ages to 1949. A very brief conclusion is followed by Evan Bukey's selected bibliography of works published by East German authors since 1978 and a biographical appendix of the GDR's leading historians. Georg Iggers's preface sets Dorpalen's *oeuvre* in its own historiographical context.

Based on a massive reading of East German literature, this book comprises an extended report of the principal conclusions of this literature on a long list of historical problems, such as the significance of the Protestant Reformation, the foreign policy of Frederick the Great, the revolutions of 1848, the development of the Social Democratic party, and the National Socialists' seizure of power. The often exhaustive survey represents the chief value of Dorpalen's book.

The book also shows significant limitations. They speak to the author's difficulties in trying to understand a historiographical tradition foreign and in many respects inimical to his own. One aspect of the problem appears in the book's structure. Although some chapter headings contain Marxist concepts, such as "bourgeois capitalism" and "monopoly capitalism," the order of topics reproduces the conventionally political structure of German history—Middle Ages, Reformation and Thirty Years' War, absolutism, liberation and Vormärz, Second Empire, Weimar Republic, and Nazism. Within this framework Dorpalen focuses on institutions, particularly political institutions, and he gives accordingly short shrift to basic research on economic questions by GDR historians such as Adolf Laube and Jürgen Kuczynski. Parenthetically, Dorpalen also omits several other important categories of writers—German-speaking Marxists whose work has no official support or sanction in the GDR (including such non-Leninists as Leo Kofler and Arthur Rosenberg), works on German history by non-German Marxists (such as M. M. Smirin), and writings by the GDR's historians on subjects outside German history. Dorpalen's announcement of these limitations in his title does not quiet doubts about the wisdom of this approach to a frankly internationalist tradition, which portrays Germany's history "not as particularly unique, but rather as part of a worldwide historical process" (p. 499).

Dorpalen cannot generate much sympathy for a historiographical tradition with such universalist claims. To a man whose own conceptions are rooted in the German-American émigré experience and in the tenets of German historicism, historical materialism must carry debilitating burdens; its claims to represent a philosophy of history must impose the bias of ideology, which threatens at every juncture to corrode the validity of the conclusions its practitioners reach. This conviction underlies the patronizing tone with which the author tests the soundness of the East Germans' conclusions by comparing them to what Western authors have written. To cite but one example, Dorpalen's discussion of the Prussian military reforms of the early nineteenth century emphasizes how GDR historians blame "crass class egoism" for the king's refusal to mobilize a mass militia against the French (p. 179). Although Dorpalen is willing to concede that Frederick William was a conservative man who listened to his even more conservative noble advisors, he chides the Marxists for failing to recognize the king's doubts about the military capabilities of a militia. As his crown witnesses, Dorpalen cites the works of Hajo Holborn, Franz Schnabel, Otto Hintze, and Max Lehmann. This list of venerables only confirms, of course, the proposition that ideological bias is difficult to escape.

Finally, a limited sympathy for, or understanding of, historical materialism has obscured some of the broader and truly interesting historiographical problems this book raises. These problems have to do with the limits on historiographical debate and with the sources of what might be called the reigning historiographical paradigm in the GDR. To what extent do the Marxist classics and their successors—Marx and Engels, but also Rosenberg and the

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German Communist party ideologues of the years before 1945—impose the boundaries of orthodoxy? To what extent has the GDR's historiography been affected by the Soviet Union's experience, especially the force of Stalin in the shaping of Marxism-Leninism? Dorpalen is no crude anticommunist, for he recognizes that the simplicity and compactness of his subject "is due not only to party or governmental directives or to the efforts of the historians themselves. On the substantive plane, Marxism-Leninism provides the framework of an all-embracing and integrating theory that fuses social, political, economic, and ideological forces into an overall system" (p. 498). Dorpalen does not see, however, that the theory itself has a history, which has powerfully and pervasively affected historical writing under its inspiration. Nor does he appreciate the penetration of Marxist elements into West German historical writing, partly from foreign models and partly through the Weberian revival, which is why he is sometimes puzzled when East German and West German historians reach similar conclusions.

Because of these limitations, Dorpalen's book may more accurately be described as an extended bibliographical report than as an exercise in historiography. Its comprehensiveness and easily accessible chronological structure will nonetheless make it a useful work of reference for a long time to come.

THOMAS A. BRADY, JR. and
ROGER P. CHICKERING
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THOMAS A. BRADY, JR. *Turning Swiss: Cities and Empire, 1450–1550.* (Cambridge Studies in Early Modern History.) New York: Cambridge University Press. 1985. Pp. xvii, 302. $34.50.

Thomas A. Brady, Jr.'s, new book deals with the policies of the south German imperial cities toward the Swiss cities, the emperor, and the princes in the fifteenth and sixteenth centuries. The policy of the imperial cities aimed first to secure their independence and freedom from secular and ecclesiastical rulers and second to establish a system of law and order that would guarantee the stability indispensable for the growing mercantile economy of upper Germany. To obtain these objectives, the imperial cities had, according to Brady, the alternative of allying themselves with the Swiss cities or with the emperor. The German and Swiss cities shared affinities of language, political institutions, and political ideals: the Swiss cities too had shaken off the rule of feudal lords and were determined to be their own lords. But, because the neighbors of the imperial cities were secular and ecclesiastical princes, not self-governing communes as in Switzerland, the establishment of the Swiss political system, the Swissification of south Germany, was not possible. Instead, the cities turned to the emperor both to secure their independence and to maintain stability in the empire. Indeed, there was a convergence of interests. The emperor financed his expansionist policies with the help of the cities and used the Swabian League to extend the Habsburg dominion in southwest Germany. The Swabian League supported the emperor's policy during the Swabian War of 1499, the Bavarian War of 1504, and the War against Duke Ulrich in 1519. After the annexation of Württemberg the Habsburgs considered the possibility of establishing institutions of central government for all their lands and thus creating a center of imperial power in south Germany. The convergence of interests between the imperial and Habsburg policy was, however, destroyed by the Reformation. Nowhere had the Reformation a more powerful appeal than in the cities. It was simply impossible for the urban oligarchs to obey the emperor's orders and stamp out the Lutheran heresy, even if they had wanted to. When Charles V threatened to reestablish Catholicism by force, most imperial cities allied themselves with the Lutheran princes in the Schmalkaldic League.

Brady presents the political history of the late fifteenth and early sixteenth centuries from a new perspective. The discussion of the origin of the alliances between Maximilian and the Swabian League in the fifteenth century is particularly illuminating. The wars around 1500 now appear as the expression of a definite policy, not just as chaotic events preceding the Reformation.

Brady presents evidence that there was talk of an alliance with the Swiss instead of the emperor. But I think he overemphasizes the significance of these few voices. The remarks on the possible Swissification of south Germany appear to me a bit fantastic. Still, the influence of Huldrych Zwingli's reformation on the upper German cities may be seen against this background.

Brady is well aware of the differences of policy of cities such as Nuremberg, Augsburg, or Strasbourg. He deals not just with policies but also with the individuals who shaped these policies. Above all, he introduces us to an institution that played a major role in the formulation of the policy of the imperial cities, the Urban Diet or Städtetag. Little was known until recently of this particular assembly of representatives of the imperial cities that gathered separately from the House of the City of the Imperial Diet. It is fair to add that Brady relies extensively on the recent publications of Robert Schmidt on the Städtetag. Still, this is an immensely interesting chapter of sixteenth-century history. An unusually interesting chapter deals with the measures taken by the imperial diet against monopolies and the suc-

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cessful intervention of the imperial cities, especially of Augsburg, in favor of the big merchant houses.

Brady presents a well-argued thesis, incorporating a large body of historical studies, especially of recent literature. Presented with a touch of humor, this dramatic story illuminates the unknown dimension of urban policies, even if the thesis of the possible Swissification of south Germany may be a bit farfetched.

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MERRY E. WIESNER. *Working Women in Renaissance Germany.* (Douglass Series on Women's Lives and the Meaning of Gender.) New Brunswick, N.J.: Rutgers University Press. 1986. Pp. xiii, 263. $28.00.

Merry E. Wiesner sets out to right a historical misconception. Since Alice Clark's influential study (published in 1919) of women's work in the seventeenth century, historians have repeated Clark's conclusion that women's participation in economic life steadily decreased in the early modern period. Wiesner investigates women's work experience between 1500 and 1700 in six southern German cities that "varied in governmental structure, economic base, population, religion, and level of economic modernization" (p. 7). In this meticulously researched book, based on a wide range of archival materials, Wiesner demonstrates that the standard notion of decline is too simple. The story is much more complex. Indeed, in craft production women's formal participation all but disappeared, and Wiesner details the successful effort by journeymen to restrict women's productive roles. But, even in this sector, wives, daughters, and widows were vital to guild life, selling its wares and clothing and feeding its workers. In contrast, the new capitalist industries opened jobs to women, which provoked, however, growing concern among city officials with "masterless" people, that is, women living alone. Other occupations recorded slow gains; the number of servants increased and women continued to be prominent in health care, although sentiment turned against their treatment of people for illnesses. Women also served as low-level officials, often with their husbands. By the end of the period, however, reference was no longer made to two officials but, for example, to a gatekeeper and his wife. Wiesner also describes the essential place of women in the retail trade and the local market.

Wiesner offers the richest empirical case to date for the shifting work roles of women in the early modern urban economy. She also seeks to develop a theoretical framework to assess the changes, which were remarkably similar in the cities she investigates. She explores a purely economic explanation (men fearing female competition for work) but finds it raises numerous other questions as does the more fruitful perspective that explains women's work cycle by reference to their family cycle. The causal factors shaping women's work become clearer only by connecting socioeconomic change and women's place in the family economy to broader political and intellectual shifts. One key lies in the political transformation of the household that accompanied the bureaucratization of early modern life. Households were private entities to be sure, but they played an increasingly important public role in record keeping. Because male-headed households were seen as proper, legislation reinforced the patriarchal household and thus sharpened the separation of male and female spheres, with profound implications for women's work. Gender analysis (that is, attention to corresponding changes in male work) could have enriched some sections of the study, which otherwise is an exemplary piece of scholarship.

JEAN H. QUATAERT
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EMILIO WILLEMS. *A Way of Life and Death: Three Centuries of Prussian-German Militarism; An Anthropological Approach.* Nashville, Tenn.: Vanderbilt University Press. 1986. Pp. x, 226. $12.95.

Nothing highlights the power of deep-seated assumptions about national histories better than their condensation into a coherent macrohistorical argument. In reading this book on Prussian-German militarism, one confronts the distillation of a dated 1920s *mentalité*—itself a product of political, social, and, indeed, cultural confrontations of the preceding half-century. Yet, one can instantly perceive the formative power of the cultural constructs of that time, even if they appear as historical artifacts. To be sure, this says nothing about the validity of the author's concept, but it shows the historicity of historical argumentation.

Militarism, argues Emilio Willems, did not come into being by the fiat of a ruler. It is a social formation—or a "cultural complex"—that evolved through a series of adaptive changes under particular environmental conditions. Following Heinrich von Treitschke, Willems traces the roots of militarism to the eastward expansion in the Middle Ages and to the Teutonic Knights, even though militarism gained its formal Gestalt only in the seventeenth and eighteenth centuries in the Prussian alliance between crown, nobility, and army. Militarism was already more than a mere organization of power; it permeated society—with the exception of the bourgeoisie—and thus established a peculiar

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course of national development. Despite its fatal political and social consequences, this course, Willems seems to imply, was still functional from the point of view of evolutionary dynamics. It reflected the "geopolitical realities" of Prussia and thus, one might presume, accorded with the logic of historical development (p. 205). The adaptability of the absolutist construct was sorely tested by the Napoleonic wars and underwent a metamorphosis with the rise of (bourgeois) militant nationalism. The period after 1890, then, saw both a radicalization of militarism, in that it now organized all of society for war, and the concurrent rise of its antidote: the growing destructiveness of military technology, which made the militarist pursuit of war dysfunctional. The world spirit left Berlin. Militarism became a "casualty of evolutionary changes" (p. 11) in the cataclysmic furor of World War II, which led to a belated but "determined rejection of the militarist tradition" (p. 205)—the subjective recognition of militarism's obsolescence. Subjective and objective (historical) logic fused: history was reconciled with the historical nation in an evolutionary leap, at least in the West, whereas the dialectical consummation of the Prussian past was postponed, presumably with disproportionately growing costs, in the German Democratic Republic.

This, of course, is a study of the German *Sonderweg* writ large, an exploration of a fatal, even if originally sensible, historical choice that unraveled over centuries. Militarism is everything that Prussia was, and Prussia was everything that an assumed liberal society is not. Prussia's calamity, though, was not necessarily its militarism, which in many ways was the ill-fortune of the space it occupied, but its rebellion against historical reason. Throughout this study, Prussia is the phenotypical other, created in a process of associative reasoning that moves from formal attributes to social qualities to national and epochal essences. Lest it be forgotten, this is a very Prussian way of reasoning. As such it may very well be considered part and parcel of Prussian militarism as "cultural complex." Unfortunately, this issue remains unresolved.

MICHAEL GEYER
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WINFRIED SPEITKAMP. *Restauration als Transformation: Untersuchungen zur kurhessischen Verfassungsgeschichte 1813–1830*. (Quellen und Forschungen zur hessischen Geschichte, number 67.) Darmstadt and Marburg: Hessische Historische Kommission Darmstadt and Historische Kommission für Hessen. 1986. Pp. ix, 901. DM 70.

Winfried Speitkamp's study is a welcome addition to the modest, but growing, literature on non-Prussian Germany during the first third of the nineteenth century. Few places deserve the term restoration more than post-Napoleonic Electoral Hesse. On recovering his territories from the dissolved kingdom of Westphalia, Elector William repudiated its laws and debts. He intended to yield as little as possible to liberal bureaucrats or noble privilege. Among his one-half million overwhelmingly agrarian subjects, a progressive urban political force did not exist. After his death in 1821 his son William II briefly flirted with reform, but he refused to summon a diet and soon relapsed into a corrupt, incompetent regime caricaturing the worst examples of eighteenth-century tyranny.

Continuing recent sympathetic evaluations of the impact of the Confederation of the Rhine, Speitkamp intends a constitutional history, not in a narrow, juridical sense but as the totality of the political, economic, and social order. Hesse is a worthwhile case. Its state and society differed significantly from Prussia and south Germany and in 1831 leaped to the forefront of the constitutional, national movement, a story only touched on here.

Required by the conditions of his restoration to reinstitute the estates constitution of *Althessen*, William I acted quickly. He called a diet in December 1814, although he broke with tradition by adding a third curia of five peasants to the historical two, a first for prelates and nobles and a second for cities. The nobles seized the initiative, trying to reassert their privileges and gain a measure of fiscal control over the lopsidedly military state. This left the elector with no alternative but to preempt the debate by coming up with his own constitution. He was also responding to state officials (most of whom had served the Westphalian monarchy) and the integrative, administrative requirements of melding his territories, some of which were new to the dynasty, into a coherent, manageable whole.

Speitkamp describes this process in exhaustive detail, which, with the background, makes up most of the volume. Themes resonate from other studies of German states during this period: complex tensions between traditionalists, reformers, centralizing bureaucrats, and late enlightened absolutist state-builders. That everything came to naught because of princely intransigence is rooted in the absence of public opinion, the dispensability of bureaucrats, the lack of a fiscal crisis, and the unwillingness of Prussia or Austria to use their influence or that of the confederation to force the elector to adhere to his treaty obligations.

In the final chapters Speitkamp projects forward the personalities and ideas of the developments of 1815–16. Except for some administrative reforms, however, the 1820s remained bleak. "Electoral Hesse of all the German states was the quietest and least receptive to foreign and subversive influences"

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(p. 435). Speitkamp correctly concludes that, even under these conditions, Hesse was not an unmitigated, antimodern restoration but was evolving within its post-1815 environment, including the confederation.

Speitkamp uses archives in Hesse, Merseburg, Munich, Stuttgart, and Vienna plus printed sources and monographs. Among his outstanding finds is a draft of the stillborn 1816 constitution; William tried to destroy all existing copies. The book's main weakness arises from these sources; although Speitkamp includes many helpful details regarding demography, agriculture, guilds, Jewish emancipation, civil administration, the military, and other matters of concern to bureaucratic absolutist states, this remains a history from the perspective of those who wrote memoranda. Although he uncovers a prehistory for the events of the 1830s, until the silent movers of society are more fully brought in, that period remains incompletely understood.

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LUDWIG BENTFELDT. *Der Deutsche Bund als nationales Band 1815–1866*. Göttingen: Musterschmidt. 1985. Pp. 498. DM 78.

After decades of neglect—more contemptuous than benign—the German Confederation in recent years has generated a growing revisionist literature. Ludwig Benfeldt's work is a contribution to that corpus; how welcome it is, however, is debatable. On the positive side, it is the first study of the Bund as a whole since Karl Fischer's *Die Nation und der Bundestag*, now a century old, and it is more objective than the unabashed diatribe of the older work. It is also a reflective study, which endeavors not necessarily to rehabilitate the Bund but to reevaluate it in the light of the twentieth-century German experience. Although admitting that no one can say how Germany would have developed without Königgrätz, the author cannot imagine a worse fate than 1945. By this comparison the Bund, in providing one-half century of peace and security without threatening Europe and by displaying periodic potentialities for constructive legislation, constituted a *nationales Band* worthy of the name.

So far so good, but then Bentfeldt soars into space, seeing through Hegelian eyes strange parallels between the "synthesis" of 1815 (monarchical principle) and that of 1945, which transcends the polarities of German empire and Nazi dictatorship and incorporates a legitimacy based on the democratic principle. In this scheme the three-way division of the German world today resembles the *Trias* of the old Bund.

Speculations of this kind are harmless enough in themselves. The trouble here is that they rest on a decrepit scholarly foundation. Instead of the many recent monographs and articles by such scholars as Wolf D. Gruner, Hellmuth Seier, Volker Press, and Helmut Rumpler (to name but a few), the bibliography is populated by old and familiar names such as Heinrich von Srbik, Ernst Rudolf Huber, and Franz Schnabel—worthy authorities, to be sure, but hardly sources of new revelations. At the other extreme the only original source used is the *Protokolle der deutschen Bundesversammlung* (1816–66), which, in condensed form and generously excerpted, provides the narrative text. To the general reader, at least in Germany, this summary of the *Protokolle* may have some interest, but the serious scholar will probably find the texts in Philipp Anton Guido von Meyer's *Corpus Juris Confoederationis Germanicae* (1858–61) more useful, even if limited to the actual resolutions of the federal diet. Bentfeldt's compendium contains the gist of the debates as well but only at the cost of omitting many issues altogether: for example, the federal army, to cite the most conspicuous object of neglect.

If the scholarly value of this book is slight, its appearance is significant all the same. The younger generation of German historians, at least in the West, has found life quite satisfactory without the experience of a national *Machtstaat*, and those who now write on the Bund more or less share Bentfeldt's favorable estimate. What makes this book different is that the author is a retired insurance actuary born in 1905 and now writing as a concerned citizen. The message is beginning to spread.

ENNO E. KRAEHE
University of Virginia

RUDOLF BERTHOLD *et al.*, editors. *Geschichte der Produktivkräfte in Deutschland von 1800 bis 1945*. Volume 2, *Produktivkräfte in Deutschland 1870 bis 1917/18*. Edited by HANS-HEINRICH MÜLLER. Berlin: Akademie. 1985. Pp. 446.

WILLIAM OTTO HENDERSON. *Manufactories in Germany*. (Europäisches Forum, number 6.) New York: Peter Lang. 1985. Pp. 174. 37.00 FR.

Between 1870 and 1918 Germany made some of its greatest strides as an industrial nation. Rudolf Berthold and his coeditors splendidly present the record of this experience in a handsome book, replete with many illustrations, maps, and statistical tables. From time to time the text is marred by references to its Marxist origins. But this is only a minor inconvenience in an otherwise straightforward, objective narration.

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For the most part this book is based on the secondary literature. It draws its information from an array of books and articles. Although the emphasis is on the industrialization of Germany, such aspects as the changes in agriculture, the developments in transportation and communication, and the contributions of science and education are also prominently spelled out. There is a good chapter on the conditions of labor during this time. Another valuable chapter deals with the location of industry. Despite the fact that numerous authors have contributed pieces to the book, the whole reads quite well. It is interesting, for example, to learn how much German development was influenced by American experiences. After all, the dominant industrial country of the time still was England. Yet, there is relatively little in this book on the competition between Germany and England. Altogether, this is a useful compendium of economic matter toward the end of the nineteenth century and in the first two decades of the twentieth century.

The second book under review is a small work by the noted economic historian William Otto Henderson. It is not quite clear why Henderson wrote this book. The manufactories depicted in it have all been described in either the German- or English-language literature. The firms with which his book is concerned all had plants that were centralized workshops in the eighteenth century. The book is not an exhaustive list of eighteenth-century manufactories in Germany. For example, it does not include the Königliche Lagerhaus in Berlin, a large firm manufacturing woolen goods at times owned by the government. It was partially a putter-out and partially a centralized workshop, as was the case with most of the manufactories that Henderson takes up. The book also includes plants that were located in the Habsburg Monarchy, in particular in Upper Austria, Bohemia, and Moravia. Here, too, many other enterprises could have been included. For example, the large cotton-manufacturing enterprise in Schwechat, on which there is substantial documentation, is not included. With the Habsburg enterprises the geographical definition of Germany is of course stretched a bit.

Lastly, a brief mention should be made of the terminological problem with which this book concerns itself, if only indirectly. Many of the enterprises described in this book were originally called factories. More recently, some have been called protofactories, and in many cases they can be included in the literature on proto-industrialization.

HERMAN FREUDENBERGER
Tulane University

ANDREAS GESTRICH. *Traditionelle Jugendkultur und Industrialisierung: Sozialgeschichte der Jugend in einer ländlichen Arbeitergemeinde Württembergs 1800–1920.* (Kritische Studien zur Geschichtswissenschaft, number 69.) Göttingen: Vandenhoeck und Ruprecht. 1986. Pp. 259. DM 48.

Community study has long been a mainstay of social, demographic, and family history in the United States, France, and England, but it has found less favor in Germany. Andreas Gestrich's book is a fine addition to the growing number of local community studies that have begun to enhance our understanding of the processes of change in nineteenth-century Germany.

Gestrich's study focuses on the history of youth institutions and socialization in the village of Ohmenhausen in Württemberg. He is particularly interested in following the conditions and culture of young people in the village as it changed from a community of smallholding peasants to a weaving community and finally became, by the turn of the century, a residential suburb largely occupied by factory workers. His careful and richly documented analysis rests on both a great range of archival sources, such as church registers, letters, and criminal records, and interviews with several of Ohmenhausen's older residents whose memories extend back to the era of World War I.

Some of his conclusions are surprising but strongly and convincingly argued. He discovers, for example, that the traditional male youth culture, centering on the institutionalized and fairly autonomous peer groups that included village boys who had left school but not married, not only survived the arrival of industrial capitalist forms of employment but may even have taken on new meaning and vitality from the new "semi-autonomy" of adolescent boys in peasant-worker families. He also suggests that the general rise in age at marriage that accompanied the crisis of immiseration associated with the early stages of economic transformation (a trend reversed only in the 1880s) extended the period of youthful semi-autonomy and made the regulatory functions of the peer groups more necessary than ever.

Throughout the book Gestrich is sensitive to the subtleties of village stratification: the central and continued role that the distribution of land played in shaping the destinies of the different classes of village youth and values embedded in village norms and family strategies. He points to the ironic persistence of land-based values and strategies long after income from the land had ceased to be the mainstay of the village economy. As Ohmenhausen was converted into a workers' suburb, land increasingly provided security against the vagaries of the wage market and a last line of defense against full proletarianization. Thus, Ohmenhausen's families were

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as apt to behave like small peasants as like factory workers.

In contrast to many earlier studies of youth culture, Gestrich's is attuned to gender differences. He takes care to spell out, for example, the differences between boys' and girls' peer groups and the different stakes that young men and women had in premarital sexual practices. He even alludes to the socialization of gender roles, although this theme is not as prominent as it might have been.

The traditional youth culture of Ohmenhausen eventually disappeared. Its persistence in the face of the dramatic socioeconomic transformations of the middle and late nineteenth century was deceptive, for it rested on contradictory and fragile social supports. As World War I decimated the ranks of Ohmenhausen's young men and the political polarization of the era made a solidarity based solely on age and village identity hard to maintain, the peer group system broke down. But, thanks to Gestrich's careful and sensitive reconstruction, we have a fine analysis of the dynamics of youth culture and socialization in an era of dramatic historical change.

MARY JO MAYNES
University of Minnesota

DETLEV J. K. PEUKERT. *Grenzen der Sozialdisziplinierung: Aufstieg und Krise der deutschen Jugendfürsorge von 1878 bis 1932*. Cologne: Bund. 1986. Pp. 442. DM 68.

Detlev J. K. Peukert provides an interesting but uneven study of attitudes toward and efforts to reform the conduct of male juvenile delinquents in Germany over a period of one-half century. Placing his work within the general context of the increased attention given to adolescence in the late nineteenth century, Peukert discusses the emergence of "toughs" (*Halbstarken*) among working-class teenagers too old for school and freed from the earlier controls of apprenticeship. Although skeptical of claims about a serious increase in crime among this age group, he does not romanticize these delinquents whose behavior violated general social norms. His main concern is to examine changing attitudes about how to rehabilitate such teenagers, especially in the German equivalent of American "reform schools."

The book outlines various changes in the criminal law, particularly around 1900, that allowed state governments greater authority to remove youths past the age of compulsory education, and in some cases even younger children, from the control of their parents and to force them into reform schools. It also discusses changing attitudes within the legal profession about the value and effects of imprisonment of youthful offenders, which culminated in the foundation after 1905 of independent juvenile courts in many German cities. Peukert examines as well the movement for establishing professional guardians for orphans and illegitimate children, who constituted a large percentage of those youths sent to reform school. In addition he briefly describes the increasing role of the state in youth welfare work as embodied especially in laws of 1911 and 1922.

The reform schools themselves, as well as their inmates, receive surprisingly little space in the book, which, despite an appendix containing fifty-four tables reprinted from other sources, remains for the most part a study of attitudes and laws. Peukert is at his best in describing the crises that beset these schools after the mid-1920s, when educators increasingly began to question their earlier optimism about the possibility of rehabilitating "degenerate" youths. A number of violent riots at reform schools, dramatized in Peter Martin Lampel's play, *Revolte im Erziehungshaus*, confirmed for the general public the limits to reeducation of this sort. With the onset of the depression, lack of money forced many reform schools to release inmates before they reached the age of majority, whether or not their education had been a success. In November 1932 an emergency decree lowered the age of release to nineteen and said that in cases in which successful rehabilitation appeared unlikely youths need not be sent to reform school. Peukert links this increasing skepticism about the ability to reeducate delinquents to later Nazi efforts to isolate or eliminate the "uneducable."

At a number of points Peukert fails to relate his topic to broader historical issues. He does not, for example, ask how the increasing concern with juvenile delinquency affected contemporary debates about "social control" in the elementary schools. He mentions in passing how this concern with demoralized youth often shaded over into a fear of socialism but does not explain why many Germans made this connection. Insufficiently explored as well is the influence of feminists on the new interest in social work with youths. Most striking, given the time limits Peukert adopted for this study, is the minimal attention he gives to the effects of World War I on juvenile delinquency and reform schools in Germany.

JAMES C. ALBISETTI
University of Kentucky

GEOFF ELEY. *From Unification to Nazism: Reinterpreting the German Past*. Boston: Allen and Unwin. 1986. Pp. 290. Cloth $30.00, paper $12.00.

Critical commentaries on recent tendencies in the writing of German history by Geoff Eley and David Blackbourn, notably their coauthored book of 1980,

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Mythen deutscher Geschichtsschreibung (an expanded English edition appeared in 1984 as *The Peculiarities of German History*), have stimulated considerable controversy, especially in the Federal Republic of Germany, and for that reason to have available a collection of articles by either of them is of interest. Eley in particular has been heavily engaged for more than a decade in what has been labeled, somewhat inadequately, the German *Sonderweg* debate. Most of the articles included in the present book were either part of that exchange or are directly related to the larger issues that it involved. Eley uses the introduction to this anthology and the short prefaces to the individual selections to respond briefly to some of his critics and to attempt to clarify positions that he believes have been misunderstood or intentionally misinterpreted. He is especially concerned to explain that he espouses a flexible Marxism and not, as has been charged, a simplistic doctrine of state monopoly capitalism. These clarifications may be relevant within the context of the debate, but at the same time the value of this particular collection is questionable. Eight of the ten articles have been published previously and most of them quite recently—one in 1974, one in 1976, two in 1978, two in 1983, and two in 1984. These essays are thus still accessible, and historians are already familiar with the main themes and lines of argumentation, many of which also overlap with the rejoinders and theses set forth in the *Peculiarities of German History*. Of the two previously unpublished essays, the first, "Hans Rosenberg and the Great Depression of 1873–96," stems initially from 1979 and is as much a continuation of Eley's dispute with Hans-Ulrich Wehler as it is a critique of Rosenberg. In the other new essay, "Army, State, and Civil Society," Eley suggests a revision that would view the Prussian army as a modernizing institution—his explication is more complex, of course—but toward the end he qualifies this proposal so far that his seriousness appears uncertain.

Eley believes that, collectively, the essays assembled in this volume reflect "aspects of a general vision of the German past," that they present a "coherent view" (pp. 1, 15), and that he would like to see the anthology judged in these respects. By these standards the volume must be given, at best, a mediocre mark. Nearly all of the selections are fundamentally review essays or extended critical remarks on the works of other scholars. Obviously, Eley is most excited intellectually when he is dissecting the procedures, assumptions, and explanations of other historians, and he usually does this with exceptional skill. However, essays in this genre—and sometimes he is aggressively polemical—are generally ill-suited for the explication of a positive image of the past. A vision may be there, but it is fragmented, embedded here and there amid the critical expositions. Eley comes close to admitting, without explicitly doing so, that he is better at knocking down the arguments of others than at building his own. This is less dominant in the article on *Sammlungspolitik*—although here too he has an antagonist—and it is noteworthy that, as Eley himself says, this is the one piece in the volume that is based on empirical research in the sources. A second difficulty in trying to find a vision, and coherence, is that we cannot always be certain of where Eley actually stands because he hedges his affirmative positions with some variation of the disclaimer that he is only seeking to provoke discussion. This emphasis on the provisional nature of a suggestion has its beneficial side, to be sure, but when it appears so frequently it may also be little more than the manifestation of a joyful gadfly syndrome. The positive vision remains elusive.

In many of the essays Eley returns to an issue that provides a measure of thematic and interpretive coherence: the need to recognize the central importance of capitalism for the development of modern Germany and to differentiate its significance and implications within specified historical contexts. In this connection Eley's Marxism becomes especially relevant, but this also raises questions. In brief, his Marxism is very much in flux. His historicist impulses, though subdued, arouse doubts in him. Other uncertainties are evident, and, apart from his admiration for Antonio Gramsci, the substance of his flexible Marxism is difficult to determine on the basis of this collection. In the introduction he explains briefly that it is an antireductionist Marxism, freed from any insistence on the causal power of the economy and of the class structure. Again, this tells us more about what his Marxism is not than what it is.

These essays nonetheless remind us that Eley is often provocative, penetrating, and persuasive and that his criticism is based on enormously wide and intensive reading. (The article entitled "Capitalism and the Wilhelmine State" is a good example.) But he can also oversimplify, make exaggerated claims, be surprisingly vague, and become almost amusingly self-absorbed. In addition, this volume appears to have been prepared in some haste, increasing my sense that its publication is premature and that the presentation of Eley's conception of German history would have been better served by taking the time to develop some of his ideas in new and more positive essays to take the place of older polemics.

VERNON L. LIDTKE
Johns Hopkins University

NORBERT SCHLOSSMACHER. *Düsseldorf im Bismarckreich: Politik und Wahlen, Parteien und Vereine.*

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(Düsseldorfer Schriften zur Neueren Landesgeschichte und zur Geschichte Nordrhein-Westfalens, number 15.) Düsseldorf: Schwann. 1985. Pp. 336. DM 42.

This study of Düsseldorf from 1867 to 1890 stands in the tradition of the dissertations on electoral geography written in the 1950s and 1960s under the direction of Max Braubach of Bonn. Methodologically and conceptually, it does not go beyond them. Norbert Schlossmacher describes the city's three main political groupings—liberalism, political Catholicism, and social democracy—and each of the lodges, sodalities, unions, presses, and parties connected with them. He delineates the impact of the Kulturkampf on the city, law by law. Finally, he recounts each of the state and national elections, poll by poll. For municipal elections we are referred to his dissertation of 1984.

Unfortunately, by carefully eschewing any synthetic narrative, Schlossmacher drains most of the life out of his intended "political biography of Düsseldorf" (pp. 7, 248). His approach is encyclopedic, and his organization, correspondingly mechanical. Since the archives are not equally fruitful for each club, law, or election, some paragraphs contain no more than a sentence, and at times the discussion becomes a running commentary on the sources. Even analytical clarity is sacrificed for topical neatness, as when we learn how Düsseldorf's lord mayor was driven from office in the chapter on the Kulturkampf but learn the most plausible reason for it—gerrymandering in the Zentrum's interest—in the chapter on elections, some thirty pages later.

Yet the author presents much fascinating information here, even though he is curiously unwilling to connect the dots that would draw the picture. When the reader does this, the landscape that emerges ought to give pause to both sides of the *Sonderweg* debate. On the one hand, we see the efficient, minutely articulated police state of traditional historiography, with the prefect of the regency operating like a CIA station chief in a foreign country, controlling a network of spies and informers who infiltrated every independent activity, however apolitical or insignificant. On the other hand, we see a vibrant polity at the grass roots, flourishing confidently amid surveillance and harassment. One of Schlossmacher's most interesting discoveries is that, by the late 1870s, both liberals and "ultramontanes" had organized sophisticated party machines, which persisted even between elections, blanketing every street in the district. Far from being confined to *ad hoc* committees of notables, as Thomas Nipperdey had asserted in his study, *Die Organisation der deutschen Parteien vor 1918* (1961), partisan politics sank deep roots among the common people and even included women. A larger proportion of the Düsseldorf electorate were party members in the 1880s than in the 1980s.

What does this tell us about imperial Germany? Schlossmacher is reluctant to speculate, interpret, or join any more general debate. Thus, he "concedes" on page 9 that general statements will be possible only when similar case studies have been done for Germany's other 396 election districts! Mr. Gradgrind could not have expressed himself more scrupulously.

Schlossmacher drops his positivism only when faced with the apparent anomaly that from 1871 to 1887 Düsseldorf was obsessed politically with an issue—the Kulturkampf—whose tangible consequences for its citizenry were few. His explanation is a rather lame continuity thesis, that Catholic "ideological" opposition was rooted in the period before 1848. But how such ideological continuity is consistent with the Progressives' hegemony in Düsseldorf in the 1860s and the Social Democratic upsurge after the 1890s Schlossmacher does not say; these eras fall outside his chosen period of study.

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HOLGER H. HERWIG. *Germany's Vision of Empire in Venezuela, 1871–1914*. Princeton: Princeton University Press. 1986. Pp. xii, 285. $38.00.

Holger H. Herwig introduces diverse theories of imperialism, presents seven chapters of meticulously crafted narrative about German diplomatic and economic actions concerning Venezuela, and concludes by examining his evidence in the context of various forms of imperialism. The author explains how Germany used its nationals to build financial links abroad, examines German-U.S. trade rivalries that led both nations to draw contingency plans for war in the 1890s, and notes that the 1902–03 blockade of Venezuela caused long-term animosities and led to the Roosevelt Corollary to the Monroe Doctrine, which Germany never recognized but was not in a position to challenge. The writer shows that to the German navy, whose leaders practiced a crude form of Social Darwinism, the status quo connoted decline, and the ability to secure new markets meant prestige.

Herwig successfully disputes the belief that Germany primarily cultivated South American customers for its armaments industry. He refutes historian Friedrich Katz's contention that Germans in South America rarely assimilated with the native population and often exhibited great disdain for their adopted homelands. In a ground-breaking chapter on the Panama Canal, he states that Germany feared that the United States would close the waterway to German merchant vessels in wartime and

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recounts how Germany's navy pursued a base in the Gulf of Mexico or Caribbean, but its government hesitated to violate the Monroe Doctrine by establishing one.

The writer provides substantial evidence for his even-handed conclusions, which shed new light on German expansionism. He asserts that German policies fell within the broad definition of imperialism and demonstrates the complexities of the concept. For example, an analysis of German policies in light of Joseph Schumpeter's "sociological imperialism" proves inconclusive, for it shows that the German warrior class attempted to maintain its power through overseas expansion, a policy not always followed by those controlling the government. The "finance capital" notion of imperialism espoused by Schumpeter, Rudolf Hilferding, and V. I. Lenin, by which industry and banks accumulate surplus profits by spreading into less developed lands, is disproved by Herwig, who demonstrates that German banks and trade and insurance companies never attained a dominant position in Venezuela. He perceives Germany as a practitioner of "depression imperialism" designed to stimulate a stagnant economy. His choice for the prophet of Germany's Venezuelan policy is Thorstein Veblen, the theorist of conspicuous consumption, rather than Lenin, the theorist of economic imperialism.

In a few places, such as in the chapter "The Military Advisor's Game," the writer's reluctance to scrap superfluous data detracts from his presentation. His otherwise impeccable scholarship is marred by his failure to consult sources on Venezuelan diplomacy in English and Spanish. A volume dealing primarily with Germany and Venezuela, and secondarily with the United States, should not be based almost exclusively on European archival materials. The author might have at least looked at the two published series of Venezuelan diplomatic documents available in libraries in the United States, pertinent references in *Foreign Relations of the United States*, and the major secondary literature in English on Venezuelan diplomacy. These materials would have validated and strengthened his conclusions and expanded his appreciation of what Germany confronted in Venezuela.

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ANDREAS KUNZ. *Civil Servants and the Politics of Inflation in Germany, 1914–1924*. (Veröffentlichungen der Historischen Kommission zu Berlin, number 66; Beiträge zu Inflation und Wiederaufbau in Deutschland und Europa 1914–1924, number 7.) New York: Walter de Gruyter. 1986. Pp. xix, 427. DM 98.

Think of Germany, and sooner rather than later you think of the "ubiquitous Beamte," the civil servant. Think of the early Weimar Republic, and you inevitably think of (among other things) strikes. And yet, as Andreas Kunz tells us, the social history of both the "Beamte" and Weimar's strikes remains to be written. But not exactly, of course, for Kunz's book provides direction in what is still largely unexplored territory in German social history.

There is, to be sure, little surprising in Kunz's work. Methodologically, it is well within what has emerged as orthodox German social history. Kunz is closer to the anatomist than to the poet; he is more interested in dissection than in evocation, and his work is essentially an exercise in retrospective positivist sociology. Kunz is concerned with groups, not individuals, and he measures almost every measurable group trait in twenty-seven tables and six figures. His civil servants are rational actors in pursuit of quantifiable ends; more than anything else, Kunz focuses on the civil servants' salary battles with the state. If readers are interested in nonquantifiable dimensions of the civil servants' experience, they will have to look elsewhere.

Moreover, Kunz's portrait of life during the Weimar Republic is familiar. The parties of the Weimar coalition face insurmountable obstacles. They inherit both defeat and inflation from the *Kaiserreich* and are blamed for both. Their efforts to appease interest groups and please the Allies only fuel inflation and outrage the interest groups. The parties of the political center disintegrate, and the civil servants, after a bout of militancy and a flirtation with the labor movement and the political Left, return to the political Right—first to the traditional Right and finally to the radical Right. Kunz's study confirms the well-known.

Nevertheless, for all its orthodoxy Kunz's work is impressive. His chronology, for example, is shrewd. He treats the entire decade from 1914 to 1924 as a single unit, marked by extreme economic stress, and is thus able to relate such diverse events as the war, the revolution, and the inflation. This approach is far more coherent than dividing the period into isolated phenomena.

His treatment of his civil servants is equally effective. A "class analysis" of the period, Kunz insists, is clumsy and simplistic. He therefore develops a complex and highly nuanced group analysis and tracks both intragroup and intergroup alliances and rivalries. He is especially interested in the translation of the civil servants from "estate" to "interest group," and his discussion is both detailed and convincing.

Particularly important is his argument that the effects of the decade-long inflation still are not known. The inflation did not simply bankrupt the middle class and polarize politics. Its effects were much more complex. The inflation and, just as

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important, the process of stabilization, spurred the formation of pressure groups, some of which benefited by the inflation, some of which did not. This fragmentation of German society, and not simply its polarization, was perhaps the most damaging product of the inflation.

Finally, Kunz's book is a pleasure to read. This might seem a surprise because Kunz himself warns the reader that some sections might be dry, and, to be sure, discussions of bureaucrats' pay-differentials and such things do lack a certain drama. Yet, as an example of meticulous scholarship, loyalty to sources, and seriousness of purpose, Kunz's study is a particularly pleasing example of the historian's craft.

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HAROLD JAMES. *The German Slump: Politics and Economics, 1924–1936*. New York: Clarendon Press of Oxford University Press. 1986. Pp. xvi, 469. $57.00.

This intelligent, ambitious book will both instruct and frustrate readers. Though quite valuable in substance, the complicated argument ought to come with a label warning, "some assembly required."

To the author's credit, he offers the most encompassing and empirically informed structural explanation to date of the severity of the depression in Germany, the inability of the Weimar Republic to master and survive the crisis, and the nature and extent of the economic recovery over which Hitler presided. In Harold James's view, the gathering ossification and politicization of the German economy, especially after 1914, made it steadily less able to meet the demands placed on it. This situation inclined the nation toward the downturn, deadlock, and destructiveness that marked the interwar period.

Fatal to the republic, according to James, was the combination of a low rate of economic growth with a high degree of political instability, since efforts to treat either problem involved interest groups in bitter distributional conflicts and governments in "a complex network of vicious circles" (pp. 108–09). For a time Weimar borrowed and exported its way out of this dilemma. But the cyclical contractions of 1928–30 revealed the accumulated dead weight of high taxes and deficits, uninnovative and overextended banks and industries, and real wage levels that had devoured gains in productivity. These burdens, augmented by financial mismanagement in Berlin, brought on the pivotal banking collapse of July 1931. It transformed a series of "partial and short-lived crises" into a cataclysm (p. 295). Neither before nor after this turning point, however, did a serious alternative emerge to Heinrich Brüning's policy of even-handed deflation. The substitutes suggested by business, agriculture, and labor organizations, James contends, amounted to irreconcilably partisan attempts to hold crumbling ranks together by loading the blame and the bill for the catastrophe onto rivals. Given this, given that "most economists . . . were in practice useless" (p. 342), and given the absence of the necessary preconditions for Keynesian demand management, namely elite consensus in its favor and popular readiness to defer to that consensus, the chancellor found himself "guiding the economy within a very narrow range of options" and being pilloried for his pains (p. 71).

In fact, the conduct of his brown-shirted successors, James claims, testifies to the constraints on Brüning. Essentially conventional in its assumptions and content, Nazi economics primarily consisted of applying Weimar's wage, tax, and economic control systems in an altered context and to escapist purposes. Hitler's regime imposed domestic political stability, largely centralized government expenditures, and froze, manipulated, or neutralized Germany's trade and capital deficits, then pumped the rearmament funds thus obtained into mostly old and already overbuilt industries and regions. Nazism thus solved one problem, unemployment, but intensified the ossification and politicization to which the German economy owed its difficulties. Because Hitler always imagined expansionism as a cure-all, conquest soon became the Reich's only alternative to economic eclipse. The "interventionist state" ended up chasing its own tail toward exhaustion, and "only the complete destruction, in and after 1945," halted this self-defeating process (p. 419).

Making this large, intricate, partly synthetic, and partly original case prompts James to perform several significant scholarly services. He presents a wealth of important statistical evidence, much of it mined from out-of-the-way sources and illumined by his impressive command of economics and finance. He also clears away some tenacious myths. The work of Robert A. Brady on "the alleged rationalization wave of the later 1920s" comes in for long overdue correction (pp. 148–49). James shows that Germans, not foreigners, were responsible for their depression, although he rejects the view that "the significance of reparations was more psychological than real" (p. 21). Brüning is acquitted of having used the crisis in behalf of big business against labor (though not, in my view, of having become excessively fixated on eliminating reparations). A noteworthy casualty of the author's research is the belief that one can distinguish between old and new, declining and dynamic, or domestic- and export-oriented industries in Weimar Germany and derive the political stances of their leaders from this cate-

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gorization. If further demonstration is needed of the corporate world's political ineffectiveness during the years James examines, this book provides it.

Nonetheless, the work has what contemporary financiers call a "downside." The uneven prose frequently lacks vigor and variety; the organization into thematic, sometimes digressive chapters leads to repetition and disconnects the argument; the analysis appears more adroit and penetrating when its object is economic rather than political reasoning and behavior; and a few passages rely on inferior or inapposite sources. Of course, small errors of various sorts crop up, for example, the misprinting of employees for employers (p. 194), the affiliation of Carl Duisberg with BASF instead of the Bayer corporation (pp. 165, 176), and the description of Heinrich Gattineau as a chemist (pp. 167, 345). Not all of business "was taken by surprise" by Franz von Papen's economic program of September 1932 (p. 184), since the chancellor had discussed its terms in advance with Carl Bosch of IG Farben. Hitler was not, at bottom, a liberal in economic matters but a libertine (p. 348).

James's depiction of interwar Germany as a stalemated society too hamstrung by heritage and habit to respond to its real problems will strike some scholars as overly fatalistic. His work does not debar the suggestion made most recently by Charles Maier (*Geschichte und Gesellschaft* 11 [1985]) that in 1931–32 a devaluation of the mark and the appearance of attempting to generate employment might have had stabilizing effects, at least in the short run, but that Brüning's ideology, not existing conditions, stood in the way. And the book could have been more clearly, tautly, and engagingly constructed. Nonetheless, James has given us a rich and comprehensive exploration of the German slump with implications for more than one historiographical and contemporary debate.

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PETER D. STACHURA, editor. *Unemployment and the Great Depression in Weimar Germany*. New York: St. Martin's. 1986. Pp. xii, 230. $37.50.

As editor Peter D. Stachura notes early on, this collection of nine essays does not aspire to present a thorough or complete analysis of the numerous, still knotty questions concerning the relationship between large-scale unemployment and the decline of Weimar Germany. Rather, what the essays have in common, he says, is a concern with "mass unemployment and the social and political responses it stimulated among social groups, the main totalitarian movements, the trade unions and government" (p. 17).

Dietmar Petzina's essay, "The Extent and Causes of Unemployment in the Weimar Republic," which immediately follows the editor's thoughtful introductory remarks, is one of the most interesting and compelling in the collection. Although he recognizes the multiple factors that helped induce Germany's unemployment crisis after 1929, Petzina uses what he calls a "paradigm of reconstruction" to help explain the extent and intensity of that crisis. He contends that this paradigm "permits a more plausible interpretation of apparently irregular processes in the labour market than the widely accepted theories of stagnation" (p. 39). Thus, he argues, the reconstruction period after 1919 was followed by a "phase of 'unfulfilled' reconstruction" between 1924 and 1929 characterized by low levels of investment. This "unfulfilled" reconstruction of the 1920s, he suggests, can be contrasted to the longer and more "expected" pattern of reconstruction in West Germany after 1945.

Petzina's essay is followed by three pieces that focus on the impact of mass unemployment on physicians, women, and young people. Michael H. Kater shows that a variety of problems in the medical profession—symbolized, for example, by a *numerus clausus* system that limited the access of younger physicians to the country's sickness funds and thus to real financial independence—persisted throughout the Weimar period and were exacerbated after 1929. The specific problems that large-scale unemployment posed for women and young people are addressed by Karin Hausen and Stachura in their contributions. Hausen shows in an exceptionally discerning and detailed analysis that, for example, the Weimar Republic's unemployment assistance programs discriminated against women and that the problem of "striking a balance in the difficult relationship between family and work," always especially burdensome for women, became particularly acute after the onset of the Great Depression (p. 81). This situation was further exacerbated by changes in the nature of women's work opportunities in the Weimar Republic. She convincingly demolishes the notion that women were somehow less directly affected by mass unemployment than men. Stachura points out in his essay that there is still a great deal that we need to learn about the effects of that unemployment on young people, especially on young workers, and in his remarks he outlines a useful agenda for future research on these matters.

The responses of the Free Trade Unions to the unemployment crisis after 1929 are surveyed by John A. Moses in a piece that is well written but breaks little new ground. Michael Schneider's detailed evaluation of state policy for creating work addresses a bit more directly some of the issues that have interested historians in recent years; so, for

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example, although taking account of Knut Borchardt's thesis, he nevertheless chides Heinrich Brüning's government for undervaluing "opportunities for stimulating the economy" (p. 173).

The volume concludes with two essays that consider, from rather different methodological perspectives, the problem of the radicalization of public opinion during Weimar's final years. Jürgen W. Falter uses aggregate data analysis in his essay, "Unemployment and the Radicalisation of the German Electorate 1928–1933," to show that the "relation between mass unemployment and Nazi electoral successes was indirect." In other words, people who voted for the National Socialist party "must have been mainly people who felt threatened by unemployment and the radicalisation of many unemployed towards the left" (p. 206). The issue of left radicalization is treated by Conan J. Fischer in the book's concluding essay, which among other things suggests that Communist success during the depression reflected an intensely felt but inchoate and often volatile radicalism among many unemployed.

Inevitably, perhaps, a book of this sort is a bit uneven, and, as Stachura himself admits, some important topics get rather short shrift. On the whole, however, this is a successful and in some ways important collection, and it does provide an interesting interim report of research currently underway in several countries. The essays are generally of high quality, and the contributions by Petzina, Kater, Hausen, Schneider, and Falter merit especially careful study.

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EBERHARD JÄCKEL. *Hitlers Herrschaft: Vollzug einer Weltanschauung*. Stuttgart: Deutsche. 1986. Pp. 183. DM 19.80.

GERHARD SCHREIBER. *Hitler: Interpretationen 1923–1983; Ergebnisse, Methoden und Probleme der Forschung*. Darmstadt: Wissenschaftliche. 1984. Pp. xi, 393.

Eberhard Jäckel is one of the more astute and aggressive historians writing in the Federal Republic today. In *Hitlers Herrschaft*, which should be read in tandem with the author's *Hitlers Weltanschauung* (1981), Jäckel has systematically fashioned a thesis that is at once bold, compelling, and provocative. No stranger to controversy, the author summarizes the positions taken in his noted 1983 Brandeis lectures and in the public forum over the last decade. According to Jäckel, Hitler's seizure of power resulted from the conflict between the "monarchists" and "democrats" for control of state sovereignty, which had been waged in one form or another since the Bismarck era. What appears to be a simple thesis develops into a blueprint for revision of the historiography of an entire generation. Chapters deal in turn with Hitler's drive to power, the consolidation of his totalitarian system, the road to war, and the Holocaust.

Within Jäckel's conceptual framework the German defeat in 1918, the Versailles Treaty, and the world depression become secondary features of a larger phenomenon. The Weimar coalition, which garnered 76.1 percent of the vote in 1919, was short-lived. Its shattering reversal in the election of 1920, when it fell to 43.6 percent, was epochal in its consequences. Republicans never again held a majority in the Weimar Republic. With the "monarchists" (German National People's party and German People's party) also unable to win a majority, an untenable situation ensued. Neither group was strong enough to rule alone, yet each was able to prevent the other from doing so. The result was the chaos that rent the republic asunder.

The victory of Paul von Hindenburg in the presidential election of 1925 took on great importance. The constitutional authority placed in the hands of the Reich president—especially Article Forty-eight and the power to dissolve the Reichstag—was crucial to the political balance. Hindenburg used these powers to lead an abortive "monarchist" resurgence under Heinrich Brüning commencing in 1930. Ultimately, Hitler's success was due as much to the scheming of the "monarchists" as to the strength of his supporters in the National Socialist party (NSDAP).

What began as a tactic for Hitler—playing the NSDAP and the "monarchists" against one another—became a leitmotif of the entire Third Reich. Jäckel argues plausibly that the "monarchists" played a major role in the repression of the Left that ensued as Hitler consolidated his power in 1933. The Führer ultimately co-opted the "monarchist" majority in his cabinet, which refused to demand compliance with Article Fifty-eight of the constitution requiring their approval of governmental initiatives.

At some points the author seems to grope for facts to buttress his theory. He argues that the liquidation of Ernst Röhm and the SA on June 30, 1934, was more an attempt to placate the "monarchists" and generals than it was a dramatic cleansing of radical excesses within the party. The firing of Baron von Fritsch as commander-in-chief of the army is viewed more as a blow to the "monarchists" than an opportunity to remove an official who stood in the way of Hitler's plans for aggressive war.

The chapter on the Holocaust is equally provocative. Jäckel rejects as specious the widely held theory that all roads in German history led to the gas chambers. Had there been no Hitler, there would have been no "Final Solution." His orders for the

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exterminations did not result from long-term planning; rather they emerged in the heat of battle and were tied to the assault on the Soviet Union. At times Hitler himself did not know how to proceed, but competition among his paladins acted as a catalyst to the Holocaust. He overestimated the resistance his plans might cause; in fact, he had very willing helpers and met less opposition than when he took the nation into war. As Heinrich Himmler stated on March 13, 1940: "I am not doing anything that the Führer does not know about."

Hitlers Herrschaft is highly recommended, if only for the many fresh avenues of inquiry it offers the historian. Jäckel is singularly unimpressed with theories of a German *Sonderweg* and attacks all models of continuity in recent German history except his own. Even the debate between the "intentionalists" and "functionalists" is made to seem irrelevant in his disarming analysis.

Gerhard Schreiber's *Hitler: Interpretationen 1923–1983* presents a laboriously prepared historiographical synthesis of the present state of research on the Hitler problem in its broadest context. Schreiber is well qualified for this assignment. In an extended series of topically selected essays, he undertakes to analyze critically fifteen hundred titles on the National Socialist era. In part he focuses on present and past controversies on a wide range of subjects. The first section, dealing with aspects of interpretation from the 1920s through 1945, represents an important contribution. Most striking is Schreiber's investigation of both German and international opinion regarding Hitler and the National Socialist movement in France, Great Britain, and the United States. Underestimation of the Hitler phenomenon was pervasive. The author deals in turn with the contemporary reaction of the Christian community, anti-Semitism and its reception, and Hitler's ideological and political aims.

The author next assesses the voluminous literature of the postwar era, organizing his analysis thematically. He treats discretely economic, constitutional, social, and ideological issues before turning to mass psychology as well as the role of individual personalities in the Third Reich. The wide-ranging theories on the fascist phenomenon—including the Marxist-Leninist interpretation—are also analyzed. Schreiber goes far toward illuminating the lines of argument of many leading historians on these issues. However, there are far too many serious omissions, especially of English and American historians, to consider this work definitive. Problems of organization abound as well. Nevertheless, this is a valuable reference volume, not least for the useful concluding bibliographical compendium on subjects ranging from "Alltagsgeschichte" to the Resistance.

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DIETER REBENTISCH and KARL TEPPE, editors. *Verwaltung contra Menschenführung im Staat Hitlers: Studien zum politisch-administrativen System.* Göttingen: Vandenhoeck und Ruprecht. 1986. Pp. 434. DM 75.

This book is interesting and unusual in a number of respects. Not only is the history of public administration, as the editors rightly note, a largely neglected aspect of Nazi studies but the list of contributors shows some unusual features. The authors in this volume include archivists, experts in public administration, and a jurist in private law practice, as well as historians from three countries. The volume has both a scholarly and a didactic aim. It provides a number of case studies discussing the relationship between party and state in the Third Reich. The editors also hope that the book will serve as a textbook or reader for graduate courses in the history of public administration.

As is true of almost any collaborative work, the individual contributions vary considerably in both quality and interest. One of the best pieces is the introduction by the book's two editors, Dieter Rebentisch and Karl Teppe. They stress the peculiar *Problematik* of the role of public administration in the context of the "dual state." It is clear that, far from standing in the way of the progressive totalitarianization of German society, the state administrators for the most part not only were willing instruments of the Nazis but, by their very existence, provided even the Nazis' most perverted policies with a veneer of formal legality. For the most part the conflict between state and party was an unreal one. State agencies usually were content to retain formal, rather than actual, authority. Cases of conflict between state and party likely as not involved state agencies that had been subverted by and now acted in the interest of a powerful party official.

Among the individual contributions to this volume, Eberhard Laux's analysis of the conflict between the Nazi concept of *Menschenführung* (leadership) and the public administrators' traditional understanding of *Verwaltung* (administration) provides a useful introduction to a theme that permeates virtually all of the case studies. Laux's theoretical analysis is particularly welcome because his discussion includes not only the ideas of Carl Schmitt but also those of lesser known, but perhaps more representative, Nazi thinkers, such as Reinhard Höhn and Ernst Rudolf Huber.

Rebentisch's article on Hans Lammers and the Reich chancellery during the Third Reich is a vivid portrayal of an able bureaucrat who put his skills at the disposal of Hitler. The process by which Lammers became the Nazi dictator's unquestioning instrument is a fascinating study in the personification of the dual state.

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In contrast, Klaus Oldenhage's analysis of an aspect of the judicial administration, Horst Romeyk's essay on the office of the district director in Düsseldorf, and Christian Kirchberg's piece on the relationship between the Gestapo and the administrative law courts are somewhat disappointing. All three suffer from excessive and irrelevant detail. They also demonstrate what happens when historians of public administration do not take account of the specific language used by their subjects. All three of these essays display an (undoubtedly unintended) tendency to whitewash the activities of the offices they analyze.

Hans Fenske's essay on Josef Bürckel and Gerhard Kratzsch's contribution on the economic advisers of the National Socialist party in the Gau Westfalen-Süd suffer from some of these same defects. Fenske's contribution concerns one of the most powerful of the Nazi *Gauleiters*, but the author's formalistic writing does not explain how Bürckel built his political machine. Ironically, Lothar Kettenacker's essay in this same volume on the chiefs of civil administration in World War II, which deals only in part with Bürckel, is far more convincing in this respect. Kratzsch's essay is almost a model of how not to write administrative history. The piece is studded with detail but provides little information on the actual activities of the regional economic adviser. Particularly unfortunate is the author's decision to exclude "for space reasons" this office's role in the expropriation of Jewish property. Here again Kettenacker's contribution provides a far more vivid picture of the economic adviser's role in Nazi totalitarianism.

Perhaps the best essay in the book is Jeremy Noakes's piece on Philipp Bouhler and the Führer chancellery. Noakes paints a vivid picture of such quasi-administrative offices and the motivation of their officials in the Third Reich. He demonstrates that Bouhler's agency became important precisely because it had no regular administrative standing. The Führer chancellery was used by Hitler when he wanted to carry out programs that he knew would be unpopular among large segments of the population. Bouhler and his associates in turn eagerly sought responsibility for such tasks, because it was their only road to power. Bouhler, who had been by-passed for one of the "big" patronage jobs when the Nazis came to power, enthusiastically administrated the infamous euthanasia program because it provided his agency with the highest form of authority available in the Nazi regime, a direct order from the Führer.

Unfortunately, Donald McKale's contribution on the party courts and the civil service, Josef Henke's essay on the SS's attempts to control the Appolinaris mineral water firm, Teppe's article on the *Gauleiters* in their role as Reich defense commissioners, Horst Gies's piece on the origins of the agricultural (*Reichsnährstand*) legislation, and Marie-Luise Recker's essay on Robert Ley's appointment as Reich commissioner for public housing are less distinguished. McKale provides little information that is not available in his earlier book. Henke's account deals with a fairly marginal item in the history of the Third Reich. Both Teppe and Gies provide a great deal of detailed information, but the larger significance of their findings is not very clear. Recker's piece would have benefited from a closer reading of Laux's contribution to this book. Her discussion of the potential efficiency of a special administration misses the point. Ley was an extreme *Unitarist*, to use Laux's term, among the Nazi party leaders. He had no interest in administrative efficiency but was determined to merge the regular state administration with the party offices under his control. For Ley *Menschenführung* and *Verwaltung* were the same thing as long as he was in charge.

The last three essays address aspects of what Robert Koehl has called the neofeudal nature of the Nazi regime. Christopher Browning's piece on Harald Turner's role in occupied Yugoslavia is of interest primarily because Turner's career demonstrated the extreme importance of personal connections in the Nazi system. Although Turner's policies were not unsuccessful, he quickly fell after he had lost Heinrich Himmler's support. Diemut Majer's contribution on the powers of the SS and the police in the occupied east and Kettenacker's analysis of the chiefs of civil administration in Eastern and Western Europe demonstrate the significance of wartime developments in the occupied areas for the evolution of the Nazi regime. The essentials of "the second revolution," which really meant the removal of the last vestiges of the *Rechtsstaat*, were deliberately tried out in the occupied areas with the clear intent of transferring these policies to the *Altreich* at the end of the war. Incidentally, Majer's essay is a useful corrective to the impression left by Henke's earlier contribution.

In general, then, this is a welcome addition to the literature on Nazism. The volume's usefulness would have been enhanced by better cross-referencing. Since many of the pieces deal with similar issues, a discussion of the views of the colleagues writing in the same volume would have helped clarify some of the issues. On the other hand, the extensive bibliography will become a useful aid for anyone doing research in this area.

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ALFRED KUBE. *Pour le merité und Hakenkreuz: Hermann Göring im Dritten Reich*. (Quellen und Darstellungen

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zur Zeitgeschichte, number 24.) Munich: R. Oldenbourg. 1986. Pp. vi, 389.

Deploring the fact that Hermann Göring (the number two man in the Third Reich) has not been the subject of a solid biographical study based on a wide range of documentary sources, Alfred Kube has fashioned this volume to fill the gap. Having conducted extensive and intensive research in German economic, political, and diplomatic records, a thorough study of the international published literature, and a more selective examination of foreign archival sources, Kube provides a detailed study of Göring's era of real power (1933–39), together with quick sketches of the earlier and later stages of his life.

Passing beyond the view frequently presented by previous writers on Göring that the Reichsmarshall was sometimes out of step with the other Nazi leaders, Kube contends that Göring's political views differed in fundamental respects from those of Adolf Hitler and his other palladins. In Kube's view Göring was not a racist party radical but a "national conservative" who, like many other such people in the German Nationalist, rather than the Nazi, party, pursued German recovery and expansion because he was a "late imperialistic revisionist politician" (*spätimperialistischer Revisionspolitiker*).

Kube rests most of his case on detailed study of the active role played by Göring in the economic sector of the Third Reich, especially during the middle 1930s, and also on a thorough examination of his effort to expand Germany's influence and economic interests in Southeastern Europe (at the expense of Fascist Italy as well as the Western powers), which extended from 1933 to 1939.

Scholarly readers will have to make up their own minds whether Göring's economic pursuits and his Southeastern European activities suggest that he was a lesser or greater expansionist and racist than Heinrich Himmler, Josef Goebbels, or Hitler. I frequently found that, although Kube's specific points usually seemed plausible, if occasionally overdone, the line he wanted to draw between Göring's basic views and those of Hitler and the "Nazi party radicals" became fuzzy in the extreme. This was especially true in regard to such pivotal matters as the pursuit of autarky and the creation of the *Anschluss* with Austria.

Through this volume Kube has performed a service by seeking to fit Göring's peculiar life and career into a coherent pattern. He has also done very helpful work in uncovering important new documentary material, especially documents related to the early consolidation of the Third Reich and the economic struggles of the mid-1930s.

But, in the end, there remains the mystery of Göring's vanity, overindulgence, inability to confront Hitler, and tendency to sink into depression when out of the Führer's favor. This, rather than clearly defined ideological differences with the Führer, one suspects, was the vital aspect of Göring's career. Kube is troubled by this problem, as was I while reading his book, but a detailed study of the personality dimensions of Göring's career still needs to be written.

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GILMER W. BLACKBURN. *Education in the Third Reich: A Study of Race and History in Nazi Textbooks*. Albany: State University of New York Press. 1985. Pp. 217. Cloth $38.50, paper $14.95.

It is difficult to see how Gilmer W. Blackburn's book makes any new contribution to historical scholarship. The title is misleading since the author's primary interest lies in the study of National Socialism as "an ersatz religion" rather than in school instruction (pp. 75, 162). His examination of history textbooks serves simply to illustrate "the secular religious character of the National Socialist *Weltanschauung*" (p. 182)—a thesis that is hardly original. The problems arising from his hazy definition of the topic, inadequate research, and tendency to digress emerge at the very beginning. The first two chapters, "National Socialism and the Modern Malaise" and "National Socialism as a Metaphysical System," are written in a pretentious style and restate old platitudes. They do not lay a good foundation for an analysis of the instruction of history in the elementary schools of the Third Reich. Apart from scattered fragments of information drawn from the works of Rolf Eilers and Kurt-Ingo Flessau, Blackburn provides no account of the changes that Hitler's regime introduced in the schools after January 1933. The absence of any discussion of the schoolteachers leaves his study of the textbooks suspended above the real world of the classroom.

The chapters dealing with education are essentially a thematic analysis of history textbooks. Blackburn shows that the subject of history, as presented in the Nazi textbooks, became a secularized religious instruction as well as a form of political indoctrination. Employing religious imagery and a reverse chronological tactic in the arrangement of the material, the writers sought to awaken in the children a reverence for Hitler and a faith in his mission and his view of Germany's destiny. Blackburn suggests in two random remarks that the outbreak of war in 1939 led to changes in the content of history textbooks. Before 1939 the authors did not give full expression to National Socialism's racial xenophobia or hostility to Christianity. Diatribes against the Jews and the Christian reli-

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gions crept into the textbooks during the war years. Dietrich Klagges's *So Ward das Reich*, which Blackburn describes as the most anti-Christian history textbook of the Nazi era, was published as late as 1943. He does not explore this comparison at any length and clearly distinguish the didacticism of the books used in the schools in the 1930s, when the Germans were first being socialized into the Third Reich. The books that provide the bulk of his evidence were published during the war.

Blackburn seems to assume that his sample of textbooks reveals what schoolteachers actually taught and what pupils absorbed. Since his research does not cover school administration records, school inspectors' reports, and the confidential reports on opinion and morale compiled by the Sicherheitsdienst and other agencies of the Nazi regime, the reader never learns how widely and in what regions the textbooks were introduced in the schools, whether the teachers presented the prescribed material, and how the parents of the schoolchildren reacted. His data are too thin to produce any convincing general conclusions about school instruction and "the regime's success" in implementing Nazi educational policy.

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INTERNATIONAL AUSCHWITZ COMMITTEE. *Nazi Medicine: Doctors, Victims, and Medicine in Auschwitz*. New York: Howard Fertig. 1986. Pp. xii, 700. $49.50.

MIKLOS NYISZLI. *Auschwitz: An Eyewitness Account of Mengele's Infamous Death Camp*. Foreword by BRUNO BETTELHEIM. Translated by TIBÈRE KREMER and RICHARD SEAVER. New York: Seaver. 1986. Pp. xviii, 160. $5.95.

Miklos Nyiszli was a young Jewish-Hungarian pathologist who was deported to Auschwitz, with his family, in spring 1944. Because he possessed unique experience as a forensic scientist, he was needed by the Auschwitz SS medical team. Eventually, he ended up as Josef Mengele's assistant, having been granted his own dissection office and laboratory facilities in the liquidation camp's crematorium. Thus, he became a constituent member of the *Sonderkommando*, that forlorn group of Jewish prisoners whose remaining function in life was to burn the victims' bodies after gassing. At the end of approximately three months, all members of the several *Sonderkommandos* were themselves killed, and a new squad was recruited. Nyiszli survived the execution of the doomed group he was attached to because he was indispensable to Mengele, who had to vaunt his scientific fancies, and eventually Nyiszli was liberated from captivity.

Nyiszli's account of life in Auschwitz was first published in English in 1960, with a reluctant foreword by Bruno Bettelheim. The book became one of the early important sources about Auschwitz, not least because it afforded valuable insights into Mengele. One of the problems with it, and one that caused Bettelheim to hesitate in composing a preface, was the individual morality of its author. Did Nyiszli not collaborate with the enemy by aiding in the pathological analysis of Auschwitz inmates who had been murdered? This question evidently also bothered the Hungarian physician at the time that he was writing his book; ultimately, he too could do no more than admit that he probably prostituted himself to save his life (and that of his wife and child, who both survived Auschwitz with him).

In 1960 it was right to publish this small volume, for the harrowing account it rendered and precisely for the moral issues it then raised. But since then we have witnessed an Eichmann trial, a Frankfurt Auschwitz trial, and countless other, smaller, court proceedings. Just recently, the death of Mengele occurred. All these events combined have generated new and fairly reliable sources of information about Auschwitz and the Holocaust as a system of genocide. Several of the moral issues have been dealt with, often in soul-searching fashion, in books such as Hannah Arendt's and now again in Robert Jay Lifton's. Why, then, republish this volume? It contains the same preface by Bettelheim, the same introduction by Richard Seaver. It has not been revised, edited, or annotated in any way. Hence old legends are allowed to resurface, such as that Mengele was chief physician in Auschwitz and that the objective of his "research" was to accelerate the propagation of racially perfect "Aryans," as was once the apocryphal message of that entertaining movie, "The Boys from Brazil." The generic origins of the term "Kapo" are definitively explained by the translators (in reality, we still do not know where the word "Kapo" or "Capo" comes from), and Bergen-Belsen is mentioned as an "extermination camp" (which, as a *Durchgangslager*, it certainly was not).

Similar doubts prevail after reading the anthology of short studies of various aspects of Auschwitz camp life, mostly medical, by Polish and Jewish authors, originally edited by Cracow surgeon Józef Bogusz and published in the Polish-based monthly *Medical Review*. The American publisher has reprinted these pieces in translation; for reasons of cost they are produced in facsimile mechanical-typewriter script and often not very legible. Specialists on the Holocaust or even historians of medicine will probably find some of these articles useful, but to the general reader in history the book has little to offer. One of the reasons for this is the unbelievably bad translation from Polish into English, the nasty consistency of which is vexing till the end. Another

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is the fact that once again these pieces, all stemming from the 1961–67 period, are hopelessly dated. It does not serve the reader to be wrongly told, once again, that Mengele was Auschwitz's chief physician (III, 6) or to be lectured that it was the Nazi party "that decided about whom to give a university chair" (I, 207), for since the 1960s research outside Poland has progressed. A distinct pro-Polish bias has led the editors to accept judgments about Hitler's Polish policy uncensored, such as his alleged sentencing of the entire Polish people to "biological extermination" in September of 1939 (III, 58). By the 1960s even Polish scholars ought to have known that Hitler was bent on extinguishing the Polish intelligentsia only and that he wished to preserve the rest of the people for slave labor. These factual mistakes and intemperate interpretations are compounded by a leaden descriptive style that might have been intended to evoke the authenticity of firsthand reports but in this case serves mainly to dull the reader's interest.

Research into Holocaust history is developing apace; enriched by social scientific and psychological methods, the techniques of inquiry are constantly refined. To be sure, emotional, demonological treatments of the Nazi system of oppression were widespread in the two decades following World War II, and to a large extent they were understandable, if never excusable. Moralistic urgency bordering on demonology characterizes both these volumes, a quality that irks me, all the more so because the books' factual underpinnings, from today's hindsight, are less than secure. Ceding recognition to a classic—and the first edition of Nyiszli's volume may well be a classic of a sort—is right and proper, but that should not necessarily have led to a new unaltered edition, warming up half-truths and possibly falsehoods. Nor does a shoddy translation of rather subjective, outdated reports beyond any annotational framework contribute to the universal search for truth, especially if it is done for no other reason than to celebrate heroes of the Auschwitz underground. What it does, instead, is compound the already massive difficulties encountered in clearing away the fog still shrouding the fate of Hitler's racial victims.

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RAINER FRÖBE *et al. Konzentrationslager in Hannover: KZ-Arbeit und Rüstungsindustrie in der Spätphase des Zweiten Weltkriegs*. In two volumes. (Veröffentlichungen der Historischen Kommission für Niedersachsen und Bremen, number 35; Quellen und Untersuchungen zur allgemeinen Geschichte Niedersachsens in der Neuzeit, number 8.) Hildesheim: August Lax, 1985. Pp. 406; 407–647; 180 plates. DM 96.

This two-volume work, the product of years of collaborative effort by six young and very talented historians, is one in a series of publications sponsored by the Historical Commission for Lower Saxony and Bremen. Rainer Fröbe and his colleagues have produced a welcome and important addition to the evolving literature on the concentration camp system and the wartime armaments industries and the developing genre of studies focusing on local and regional circumstances and conditions in the experience of the Third Reich. In this particular study the local and regional factors conditioning the construction of concentration camps in Hannover during the last two years of World War II are woven expertly and effectively into the larger and more familiar themes of slave labor in the armaments and war industries of Nazi Germany. The result is a good deal of new and detailed information, as useful to the general student as it will be valuable to the specialists in the period.

Technically, this is an extremely fine book. It is based on research in a wide range of manuscript sources in archives in Germany and all over Europe and North America. It contains an extraordinary compilation of interviews with survivors of the slave labor camps in Hannover, many of whom came from the larger and more horrible camps and extermination complexes. It is generally well written and supplemented with helpful charts, diagrams and tables, a first-rate bibliography, and a stunning collection of concentration camp photographs and prisoner art. In this latter respect alone the book is equal in quality to some of the photo histories and collections of camp art published in recent years.

The subject matter of the two volumes in the study is well organized, beginning with background chapters detailing the development of the concentration camp system and the use of the extralegal process of protective custody for forcing work from those confined indefinitely to the camps. By the outbreak of the war in 1939, this tradition had become the foundation on which the gigantic slave labor enterprises of these SS facilities rested. By 1943, when the tide of war turned inexorably against Hitler, the scope of the enterprises, which had an irreversible and expanding momentum of their own, led to the rapid multiplication of networks of satellite concentration camps, created from and tied to the larger more permanent facilities but located specifically and staffed deliberately with SS guard personnel and with prisoners who could fulfill the slave labor demands of the gigantic war industries.

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This study details that process, camp by camp, throughout Hannover between 1942 and 1945. With individual, extensive chapters dealing with the history of each of these concentration–slave labor camps, the study carefully documents the creation and growth of the camps at Stöcken, Misburg, Langenhagen, Limmer, Ahlem, and Mühlenberg, each of which was specifically tied to a different industry vital to the German war effort.

These satellite concentration camps, until now largely unknown even to specialists in the history of the SS, were smaller copies of the larger, older installations—Neuengamme, Buchenwald, Mauthausen, and Auschwitz, to mention only four—from whence came their guards and prisoners. The satellite camps were created with the same SS administrative structure, the same disciplinary and punishment regulations, the same physical layout and security precautions, the same patterns for working and abusing prisoners to debilitation and death, and the same hideous procedures for torturing and murdering inmates who could not or would not work, or who offered the slightest signs of independence, defiance, or resistance. Whereas the pattern in the big camps before 1943 had been to bring the industrial enterprise to the inmates, the proliferation of these smaller, local camps reversed the process to move the slave laborers to camps adjacent to industries.

This is an extended, localized history of the concentration camps during a critical period. It is written at a level lower and is therefore more revealing than previous studies, for it unmistakably shows how, in the experience of one state and region of Germany, the proliferation of concentration camps made the facilities, and what went on inside them, a routine and familiar fact of life, both to the companies exploiting prisoner labor and to the communities in which camps and industries came to be taken for granted by a population that saw nothing amiss or unusual in a system of industrial production based on a vast edifice of human destruction.

This is an important book, the kind of model study that, one hopes, will continue to stimulate works of similar focus and quality in an area of the Third Reich only now beginning to open.

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WALTER LAQUEUR and RICHARD BREITMAN. *Breaking the Silence*. New York: Simon and Schuster. 1986. Pp. 320. $17.95.

The story of how news of the Nazis' intention to exterminate European Jewry reached the Allies in the West is known in detail largely because of the work of Walter Laqueur. (See his *The Terrible Secret* [1980].) The story is basically this: early in August 1942 Gerhart Riegner, a German-Jewish refugee living in Switzerland and a representative there of the World Jewish Congress, sent telegrams to the American State Department and the British Foreign Office informing them that he had learned from a reliable German source of Hitler's decision to exterminate the Jews of Europe.

Riegner requested that the State Department convey this information to Rabbi Stephen Wise, a prominent leader of American Jews and the president of the World Jewish Congress. Perhaps, Riegner hoped, Wise might be able to mobilize Allied actions that would at least inconvenience the implementation of Hitler's order. But about his German informant Riegner said very little, only that he had close connections with high-level Nazi officials and that his earlier reports on the situation in Germany had been totally reliable. The informant obviously did not want his name used in any written communication for fear of falling into the clutches of the Nazi police.

Partly because Riegner himself felt honor-bound not to disclose the identity of his informant, it has been wrapped in mystery for over forty years. Scholars have until now come no closer than establishing that it was probably an important German industrialist. In this remarkable piece of historical detective work, which the authors cast as a biography, Laqueur and Richard Breitman have managed to identify Riegner's informant as Eduard Schulte (1891–1966), the chief executive officer of Giesche and Heirs, a major mining firm that had its headquarters in Breslau.

Schulte, a political conservative and ardent anti-Nazi, was well connected to high-level officials in the Third Reich and, therefore, privy to many of its secrets. Business matters alone regularly took him into ministerial offices in Berlin. He also had a cousin who was an official in Colonel Hans Oster's department of the *Abwehr*, the veritable general staff of the opposition to Hitler. A friend in the Foreign Office served in liaison work with the army command, placing him in an excellent position to provide intelligence on Hitler's war plans. These contacts enabled Schulte to act on his anti-Nazi sentiments by delivering warnings to the Allies about Hitler's coming moves against both Poland and the Soviet Union. These warnings Schulte delivered during what were ostensibly business trips to Switzerland.

The most spectacular news ever to reach Schulte, however, came in late July 1942, when he learned of Hitler's scheme to exterminate European Jewry in its entirety. How Schulte discovered this "terrible secret" and the difficulty he had in conveying the news is fascinatingly reconstructed by the authors. The story reaches from Heinrich Himmler, through

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Gauleiter Karl Hanke, to Otto Fitzner, Schulte's number-two man at Giesche and a committed Nazi whose discretion was regularly overpowered by his desire to impress people with what he knew. Schulte was among those whom Fitzner sought to impress.

Why has it taken so long to uncover Schulte's identity? That story is no less fascinating than Schulte's. In an epilogue the authors describe the tortuous route by which they managed to uncover Schulte's name. Their description is a case study in historical detection and probably should be read before reading Schulte's story itself.

Although the authors make no mention of this, their book argues persuasively, if only by implication, that Hitler did in fact deliver an order calling for the Final Solution.

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JAMES M. SASLOW. *Ganymede in the Renaissance: Homosexuality in Art and Society*. New Haven: Yale University Press. 1986. Pp. xvi, 265.

Ganymede, the shepherd boy abducted by Jupiter to become his cup-bearer and lover, is the subject of at least two hundred works of visual art surviving or recorded in the Renaissance and early baroque periods. In this provocative book art historian James M. Saslow explores the visual representation of the Ganymede legend, from Alberti's discussion of the myth in 1435 through the exhaustion of the motif, as represented in the work of Rembrandt and Rubens, two centuries later.

Contrary to the publisher's subtitle, which promises more than it can deliver, Saslow has not tried to write either a comprehensive study of homosexual themes in Renaissance art or a large-scale study of homosexuality in Renaissance society; the Ganymede topos, after all, is only capable of expressing the particular relationship between an older man and his younger partner. And a myth so inextricably tied to the history of Neoplatonism cannot represent the homosexual experience of peasants, artisans, and others socially excluded from those cultural references: Saslow's study is of high culture, and male high culture at that. Nonetheless, as the most appropriate symbol of male-male love during the period, the Ganymede myth provides a powerful tool for exploring the relationship of sexuality and gender to issues of visual representation.

Representations of Ganymede reached an apogee in the period 1525–45, and the myth nearly disappeared from Italian art as a major subject following the Trentine decrees. The study is divided into five chapters, each of which examines a single artist (or in one case two artists) in whose work the subject of Ganymede figured prominently: Michelangelo (1475–1564), Correggio (born 1489 or 1494), Parmigianino (born 1503), Giulio Romano (born 1499), and Cellini (1500–71). In the work of these artists, Saslow argues, the rape of the handsome adolescent resonates with homoerotic energy. By the time of Rembrandt (who portrayed Ganymede as a bawling, pissing baby terrified by his abduction) and other seventeenth-century Northern European artists, Ganymede had become a desexualized infant, an iconographic transformation appealing to heterosexual, family-oriented sentimentalism.

Saslow's exposition is elegant and subtle. Interesting sexual, social, and cultural juxtapositions abound. He compares treatment of the myth as mediated by differing experiences of male sexuality (heterosexuals such as Parmigianino, Corregio, and Romano; homosexuals such as Cellini and Michelangelo) and of differing degrees of homosexual identity (the inhibited Michelangelo, the uninhibited Cellini). He compares early versions of Ganymede in Florence, representations of sexuality idealized and sublimated under the impact both of Neoplatonism and of Savonarola, with the sensual representations commissioned in the libertine court circles of northern Italy.

Saslow successfully explores the role of patronage, as well as the relationship between literary and visual treatments of Ganymede, in the various Italian courts. The popularity of the Ganymede topos has important implications for understanding certain social relationships (master-servant, noble-page, artist-apprentice) during the period. It also, as Saslow persuasively argues, demonstrates that "knowledge and appreciation of the long tradition of homoerotic art and literature were not confined to those with 'special interests'" (p. 16).

Premodern culture remains unintelligible as long as we fail to understand how the social construction of sexuality—homosexual as well as heterosexual—operated within it. In bringing insights from feminist studies, the psychological interpretation of art, and, especially, gay studies to the subject of Renaissance culture, Saslow has made an extremely important contribution. This book deserves a wide audience.

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THOMAS FRENZ. *Die Kanzlei der Päpste der Hochrenaissance (1471–1527)*. (Bibliothek des Deutschen Historischen Instituts in Rom, number 63.) Tübingen: Max Niemeyer. 1986. Pp. vii, 562. DM 143.

Thomas Frenz offers us a treasury of information in this important book. Using over eight hundred

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papal documents gathered from various German archives, Frenz explores the bureaucracy that generated them. In doing so, he presents the fullest and clearest exposition of the workings of the apostolic chancery to date. He tells us that he interprets the term "apostolic chancery" in its broadest sense: "I understand this to include all employees and institutions of the curia that had anything at all to do with the expedition of documents that were issued in the name of the pope" (p. 40).

The work is divided into three major parts. The first section (four chapters) deals with the documents themselves—their origins, classification, preparation, passage through the curial offices, and final expedition as simple briefs or as letters "under the lead" (*sub plumbo*). The second section (four chapters) is a detailed description of the offices themselves and the employees who filled them. The third section is an alphabetical and biographical list of 2,223 officeholders (pp. 261–455). This central part of the book is preceded by lists of the original documents and followed by appendixes of unpublished materials, tables listing functionaries of the various offices, and exhaustive indexes.

Frenz confines his study to the period between the accession of Sixtus IV in 1471 and the sack of Rome in 1527 and does so for good reasons. It was precisely during this era that the papacy, beginning with Sixtus, elevated the sale of curial offices ("venal" offices) into an organized and systematic method of acquiring short-term revenue. The numbers of venal offices steadily proliferated, their prices as steadily increased, and, as Frenz asserts, these circumstances had serious consequences for the future. First, from 1471 on, the bureaucracy in Rome doubled and tripled in personnel—from about two hundred employees in 1471 to some seven hundred in 1527, and this was only the beginning. Second, the system encouraged the accumulation of offices by individual officeholders and their resale to third parties. Third, the system encouraged nepotism—if indeed it needed encouragement—as ambitious "curial families" emerged. Finally, the system rendered any reform of the curial bureaucracy impossible.

These are not new or startling conclusions, but I have never seen a work that proved them so graphically. The overwhelming amount of data, the many carefully designed tables, the meticulousness of the presentation make the book a masterpiece that will be indispensable to anyone working on the fifteenth and sixteenth centuries. Finally, the book is handsome and sturdy, beautifully produced. My only regret is that it did not appear ten years ago.

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RICHARD KRAUTHEIMER. *The Rome of Alexander VII, 1655–1667*. Princeton: Princeton University Press. 1985. Pp. xiv, 199. $25.00.

Following his fundamental work on the Roman basilicas and many other distinguished publications, Richard Krautheimer's new book on the Rome of Alexander VII comes as a special event. It succeeds his *Rome: Profile of a City, 312–1308* (1980) and singles out the pontificate of Alexander VII (1655–67). Had the author carried out his more extensive plans to cover the city in three volumes he would have had to examine a considerably wider context, which would have extended the "profile" to 1700. But, because "none of the great building popes" shaped the center of Rome more than Alexander VII and because, as Krautheimer emphasizes, this pope also created a "new image of Rome," he deserves special attention. The purpose of Krautheimer's book is to demonstrate that Alexander's goal was more comprehensive than to adorn the city with beautiful piazzas.

Chapters 1–3 are introductory; they tell us about the pope's humanistic background, his specialization in canon law, his experience as a nuntius in Cologne and as a representative of the Holy See at the peace negotiations in Westphalia (1648), which ended the Thirty Years' War (chap. 1). We are also made aware of the predominantly medieval appearance of Rome in the seventeenth century, with its narrow, curved streets and unadorned living quarters. The monuments of the Renaissance and certain attempts to regularize the urban center were isolated events and had little effect on the overall face of the city (chap. 2). Finally, the reader is made familiar with the pope's architects: most important, Bernini (whose position is appropriately likened to that of a "surintendant des bâtiments" under Louis XIV in Paris), rivaled only by Pietro da Cortona and less so by Borromini (who found little favor with Alexander) and Carlo Rainaldi, younger than Bernini by about half a generation.

In chapter 4, "Teatri I: Piazze and Churches," Krautheimer discusses squares such as the Piazza Colonna—for a time destined as the new location for the Fontana di Trevi—the Piazza of S. Maria della Pace, and of course, the square of St. Peter's. He concentrates on their theatricality, that is, the conceptual affinity between temporary and permanent architecture resulting on an urban level in the scenographic display ("mostra") of churches, palaces, fountains, and so on. Roman squares and monuments have of course been examined under such aspects before, but Krautheimer does so with particular focus. Basing himself on entries in the diary of Alexander VII, he is able to confirm and enlarge our notion of Alexander's competency in architectural matters and the role of the pope as the

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initiator and driving force behind numerous urban projects that seem to have been constantly on his mind: a model of the city stood in his bedroom (p. 79).

Chapter 6 "Teatri II: Prospects," primarily concerned with straight-lined streets and their focal points as backdrops, contains a concise discussion of the appropriateness and visual consequences of the projected avenue (conceived before and after Alexander VII), which would have led from the Ponte S. Angelo to St. Peter's. Among the many objections to the project (which was not carried out until 1938) was the astute perception of a contemporary, Cardinal Palotta, that the basilica itself "because of the distance, would shrink to nothing," as can now easily be verified (p. 91).

The Piazza del Popolo (chap. 8) is of course one of the most informative examples one can find for the creation of scenic effects on an urban scale. The twin churches flanking the Corso are placed on diagonal axes that converge at the Porta Flaminia to meet the visual requirements for the spectator entering the city. One faces them in much the same way that a stage set is perceived by a prince from his centrally located box.

Again, Krautheimer places the role of the pope as the initiator into proper perspective by referring to an entry in Alexander's diary of October 6, 1656—the first record of the project. Krautheimer's statement, however, "that construction was started in earnest not before 1673" (p. 129) needs correction, because S. Maria di Monte Santo by that time had already reached the level of the window sills of the drum.

Krautheimer does not ignore the shortcomings and setbacks of Alexander's building program (chap. 9). Quintessentially, they were connected with the state of the economy and the widespread poverty of the population. But the pope argued that he provided work for many—which was in fact the case—and that it would be a mistake to support with alms those who were able to work. He also believed that the improved city would attract "large and illustrious numbers of foreigners" (p. 80).

In the last chapter, "City Planning and Politics" (chap. 10), the author provides his reader with the key to the understanding of the objectives Alexander pursued in the realization of his building program. Perplexing as it might seem at first sight, it was geared to the foreigners visiting the city (including those coming from Protestant countries) rather than to the permanent residents. Krautheimer observes that "most of the *teatri* lie along [or at least nearby] the official route travelled by important visitors," which leads in a long loop from the Piazza del Popolo to the square of St. Peter's. Entering the basilica, one's attention is almost immediately attracted by Alexander's elevated shrine of the *Cathedra Petri*, perspectively framed by the twisted columns of Bernini's *Baldacchino* (p. 138). The message the pope wished to convey is apparent: after the defeat of the Catholic cause in the Thirty Years' War and the diminishing of papal political power, Alexander had reason to stress the apostolic origin of the church as well as his claim by tradition to be a direct successor of St. Peter, on whom the church was founded. To demonstrate this claim on a broad urban scale, Alexander transformed the still largely medieval center into a magnificent modern city—to make it reflect, in Krautheimer's words, "the majesty of the Church of God" (p. 142). Such, then, were the intentions of Alexander's campaign for a renewal of the "Eternal City," whose story Krautheimer presents in a truly fascinating and insightful fashion.

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MARY GIBSON. *Prostitution and the State in Italy, 1860–1915*. New Brunswick, N.J.: Rutgers University Press. 1986. Pp. xi, 297. $38.00.

Mary Gibson has joined the small but distinguished group of social historians who are finally investigating the peculiar institution of prostitution with the tools of rigorous scholarship. Correcting the ignorance, prejudice, and overgeneralized studies of the past, she has made an important contribution to the social history of the Kingdom of Italy, as Judith Walkowitz has done for Victorian England and Alain Corbin for modern France. Gibson's principal question is "why the government of the new Italian state instituted regulation [of prostitution] in 1860 and why the policy persisted despite its failings" (p. 2).

Cavour and his successors answered the first part of this question by assuming, along with the rulers of many other Western nations, that prostitution was a medically hazardous but socially necessary institution. Thus, it had to be tolerated under strict regulation by the state. "Regulation" in practice meant police surveillance and mandatory medical examinations of the prostitutes (but not their customers) to reduce the incidence of venereal disease.

This system of regulation was modified several times after its initial enactment in 1860, but Italy remained a regulationist state up to 1958 when parliament closed the "tolerated" houses. In the period from 1860 to 1915, police surveillance was gradually separated from medical examinations, and a greater emphasis was placed on voluntary, confidential medical treatment for all those afflicted with venereal disease. Critics of regulation acknowledged that these were positive steps but continued to demand that the entire system be abolished,

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because, they charged, it degraded poor women and failed to reduce the threat of contagion.

Gibson agrees with the "abolitionists" and cites extensive evidence from government records to support their case. Most prostitutes evaded the hated regulation system, but it hung like a sword over the heads of working-class women often presumed to be prostitutes by the police merely because they were alone on the streets at night. The supporters of regulation took no account of the fact that many (if not most) prostitutes moved in and out of their profession according to the opportunities of the moment. Indeed, prostitution "represented one choice among the small range of unappealing jobs available" (p. 128). The system survived in spite of its failures because it reinforced upper-class assumptions about the natures of men and women. Women could be divided into two groups: the "passionless" angels of the house who had to be protected from unquenchable male desires and the innately delinquent fallen women who satisfied those desires.

Convincing as her argument is, Gibson tells us little about one significant aspect of prostitution. As she acknowledges, the regulation system rarely bothered the higher class prostitutes who practiced their profession in the more exclusive sections of the cities. A complete history of prostitution and the state should tell us more about these women and those who protected them.

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BRUNO SCHIMETSCHEK. *Der österreichische Beamte: Geschichte und Tradition*. Vienna: Geschichte und Politik or R. Oldenbourg, Munich. 1984. Pp. 244.

With considerable justification Bruno Schimetschek writes in the foreword of this book that the empire of the Habsburgs owed as much to the accomplishments of its civil officials as to those of military men. For over three centuries an army of hard-working, efficient, and honest bureaucrats overcame formidable obstacles in providing stability and good government to Europe's most geographically and ethnically diverse state. Himself a former senior Austrian judicial official, the author presents us with a compact, useful, and thoroughly sympathetic survey of the bureaucracy's evolution from the monarchy's medieval roots to its end in 1918.

The book unfolds chronologically. Its first half dutifully, if somewhat mechanically, treats medieval and early modern developments, giving due emphasis to the key contributions of Maximilian I, Maria Theresa, and Joseph II. Yet the author does his best work in his coverage of the monarchy's last hundred years, offering some refreshing insights and displaying a deeper knowledge of his subject. For example, the chapter on the Vormärz disputes the conventional view of iron-fisted officials zealously carrying out Klemens von Metternich's reactionary policies, arguing instead that the civil service represented all shades of the political spectrum including exponents of liberal and even socialist ideas. The bureaucracy was generally sensitive to the latest economic theories and at all levels included a large number of literati who significantly contributed to the monarchy's cultural life. Far from discouraging the spread of new ideas, the regime went out of its way to encourage and support its large number of writer-bureaucrats with sabbaticals and lower work loads. Indeed, the author suggests that, although they remained unswervingly loyal to their emperor, Austrian civil servants from Metternich down through the ranks recognized the need for reform but simply did not know how to effect change without destabilizing society.

The bureaucracy earns the author's highest praise in the closing chapter on Francis Joseph. Although himself a tireless administrator and dedicated state servant who held over one hundred thousand audiences, the old emperor increasingly relied on his civil service to overcome the national animosities that had paralyzed the Reichsrat. Nearly one-half of his ministers and over one-half of his minister-presidents were *Beamtenminister* drawn from a civil service that had grown to four hundred thousand by 1910. So great was the legacy, says the author, that the successor states that followed the monarchy turned to these same "impartial, incorruptible, professional, and dedicated" men after 1918 (p. 220).

Although the book represents a useful synthesis of an important subject, it is not without its limitations. Notwithstanding the Austrian bureaucracy's well-deserved reputation for honesty and efficiency, the author does not fortify his praise either through systematic investigation or by comparison with the bureaucracies of other states. Since he relies exclusively on secondary studies, much of the text tends toward a pedestrian survey, often consisting of nothing more than a succession of one- or two-paragraph biographies of the principal figures of the central administrative apparatus. He makes no attempt at a prosopography of the bureaucracy as a whole and thus fails to provide us with an educational, cultural, or social profile of Austrian civil officialdom. Finally, his research also fails to integrate any of the social science literature on bureaucracies and bureaucratic behavior.

CHARLES INGRAO
Purdue University

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EDWARD TIMMS. *Karl Kraus: Apocalyptic Satirist; Culture and Catastrophe in Habsburg Vienna*. New Haven: Yale University Press. 1986. Pp. xv, 443. $32.00.

Karl Kraus remains one of the most controversial writers of the twentieth century. He has been hailed as Europe's finest satirist since Jonathan Swift, he has been invoked as an ally of Ludwig Wittgenstein, he has been applauded (and derided) as a critic of psychoanalysis, and he has been belittled because his preoccupation with Vienna baffles many readers. Most scholars discern in Kraus discrepancies between his private life and his doctrines, between his early, middle, and late periods, or between his obsession with Vienna and his claim to address all humanity.

In this first volume of a planned two-volume biography, Edward Timms has gone further toward clarifying these contradictions than any previous scholar. Virtually every one of Timms's chapters attempts to resolve one or another contradiction in Kraus's lifework. Following three fact-crammed introductory chapters, chapter 4 disentangles Kraus's praise of female amorality from the misogyny prevalent during the 1890s. Kraus emerges not as a supporter of Otto Weininger's disparagement of women but rather as one who, like Germaine Greer, would direct women's alleged sensuality into a quest for harmony between the sexes. Chapter 5 distinguishes Kraus's unceasing respect for Sigmund Freud from his detestation of Freud's followers. Chapter 6 explores Kraus's lifelong friendship with the architect Adolf Loos to show that, like John Ruskin, both men excelled at "the symbolic decoding of trivia" (p. 123). Timms argues that Kraus's critique of "facades" in morals delves much deeper than that of Loos in architecture.

Chapter 7 raises the touchy issue of anti-Semitic motifs in Kraus's denunciation of pre-1914 newspapers. The book purports to distinguish Kraus's critique of journalists from his linking the mass press with its Jewish owners. Chapter 9 tackles the question of gaps between Kraus's private life and his persona as a satirist. Timms shows how as a writer Kraus enlarged his repertory of "antiroles" yet at the same time pursued astonishingly deep personal attachments. A similar gap between satire and personal life also characterized Erasmus, Alexander Pope, and Swift (pp. 244–45). By an act of will Kraus managed to keep his friendships and his lifelong love for Sidonie Nadherny (begun in September 1913) from overtly influencing his productivity. Throughout, Timms uses letters by and to Kraus to portray Kraus the private person as separate from Kraus the author. This distinction culminates in chapter 22 with Timms's insistence that the figure of the Grumbler in *The Last Days of Mankind* is a "simplified version of Kraus's satirical self" rather than a "mirror-image of the author" (p. 391).

This book offers a revision of most assumptions about Kraus. Timms invokes the most subtle psychological interpretations without endorsing any one of them. He prefers literary common sense to either psychological jargon or recondite rereadings. When he notes that in conversation Kraus preferred the simplest way of saying things, Timms could be speaking for himself. After reading this book, non-Viennese can no longer complain that the targets of Kraus's satire are too local to concern them. The author places Kraus's bêtes noires in a context that elevates both them and him. The result is one of the richest compendia we now have about the culture of pre-1918 Austria.

WILLIAM M. JOHNSTON
University of Massachusetts

JOSEF KALVODA. *The Genesis of Czechoslovakia*. (East European Monographs, number 214.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. Pp. viii, 673. $60.00.

This book provides a fresh look at the Czechoslovak movement for independence and the establishment of the Czechoslovak republic during World War I and at the Paris Peace Conference.

Czech and Slovak historians distinguish between the Czechoslovak independence movement abroad (*zahraniční odboj*) and at home (*domácí odboj*). The former is basically the story of how Thomas G. Masaryk (1850–1937), the sole representative of the Czech Realist party in the Austrian Reichsrat, gambled on an Allied victory and left the Habsburg Monarchy in December 1914 to work in exile for Czechoslovak independence; how he and his associates, Edvard Beneš and Milan R. Štefánik, organized "legions" of Czechoslovak volunteers in Russia, France, and Italy to fight with the Allied armies; how he secured Allied recognition of the Czechoslovak National Council in Paris of which he was chairman; and how he returned to Prague to become Czechoslovakia's first president. The story is fairly well known abroad, principally from English, French, and German translations of Masaryk's and Beneš's war memoirs.

In describing to one of his biographers, Emil Ludwig, how he envisioned the establishment of independent Czechoslovakia during the war, Masaryk said that he had expected to return from exile at the head of the legions, accompanying the Allied armies under Marshal Ferdinand Foch in their march to Berlin as far as Dresden, then turning south to Prague, where he would proclaim a dictatorship, which he thought necessary until the

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Czechoslovak state was properly launched. Thanks to the success of the independence movement at home, however, the establishment of Czechoslovakia differed considerably from the scenario he had envisaged for it in exile.

The story of the independence movement at home is little known abroad. Essentially, it is the story of Karel Kramář (1860–1937), the leader of the Young Czech party in the Vienna Reichsrat, and other Czech and Slovak politicians who stayed at home, gambling on liberation by Russia. They were to be disappointed, however. Russia was defeated by the Central Powers, swept by revolution, and forced to conclude a separate peace. In 1915 Kramář was arrested by the Austrian authorities, tried for treason, and condemned to death. However, he was amnestied in 1917 by the young Emperor Charles and in 1918 helped organize the Czech National Committee in Prague, becoming its chairman. When the Austrian monarchy began to crumble, the Prague National Committee declared Czechoslovak independence (October 28, 1918) and later transformed itself into a provisional ("revolutionary") National Assembly, which elected Masaryk (at the time still in Washington) president and appointed a government with Kramář as prime minister (November 14, 1918). When Masaryk returned to Prague from exile (December 21, 1918), he found a government and parliament functioning almost normally. There was no need for his dictatorship to launch the Czechoslovak state. By the time the Paris Peace Conference opened on January 18, 1919, the Prague government was in control of almost all the lands it was to claim. The peacemakers were thus faced with a *fait accompli*, which they confirmed with only minor modifications.

Josef Kalvoda tells the story of Czechoslovakia's genesis in great detail, on the basis of an extensive documentation both archival and published. He gives due attention to the independence movement both at home and abroad, stressing that both were needed to gain Czechoslovak independence. In the controversy that embroiled Masaryk and Kramář after the war over the respective merits of the independence movements at home and abroad, Kalvoda tends to support Kramář's point of view but gives Masaryk full credit for his prodigious achievement. Kalvoda's book constitutes an important contribution to European history in the period of World War I and the Paris Peace Conference.

VICTOR S. MAMATEY
Athens, Georgia

JOE C. DIXON. *Defeat and Disarmament: Allied Diplomacy and the Politics of Military Affairs in Austria, 1918–1922*. Cranbury, N.J.: Associated University Presses for the University of Delaware Press, Newark. 1986. Pp. 167. $24.50.

Writing on a subject that has received little detailed attention, Joe C. Dixon provides a competent study of military-political issues in Austria just after World War I. He builds eight short chapters primarily around the interaction between conditions and developments in Austria, on the one hand, and the policies of the victorious Allies, on the other. The author does not clearly state his thesis, but a general argument emerges from the book, although not the one indicated in the preface and introduction.

In those early sections Dixon asserts, as he implies in the title, that his book will deal with disarmament. He cautiously claims that "insight gained by subsuming Austrian military affairs under the general theme of disarmament promises to clarify, if only a little, the tragic saga of Europe's attempt and failure to make peace with itself after the self-destruction of the Great War" (p. 8). The story of events in Austria does illuminate the forces that prevented the pacification of Europe after 1918, and Dixon explores the politics of military affairs in Austria—including largely ineffectual Allied efforts to destroy military supplies and to disarm the civilian population—cogently and as comprehensively as possible in a suitably short book. But the book deals primarily with political controversies surrounding the creation of a new army, not with disarmament per se.

In developing his argument, Dixon examines two areas of conflict: that between rival political groups within Austria and that between Austrian and Allied officials. His basic argument seems to be that the Allies forced the Austrians to create a kind of army that no political camp in the country wanted, that is, a long-term professional one instead of a short-term militia or conscripted force. A corollary but less convincing contention is that had the Austrians been left to their own devices probably "a *modus vivendi* between the existing *Volkswehr* and emerging *Heimwehr* could have developed" (p. 79). Dixon undercuts the basis for such a contention, however, by emphasizing the virtually irreconcilable ideological hostility between Left and Right in Austria and the polarizing tendencies throughout East-Central Europe.

Unfortunately, the army question was not the only major issue dividing Austria's conservatives and socialists. Disputes over several other key issues kept the two camps from reaching a mutually satisfactory compromise about military policies and prompted the rivals both to compete for control of the new army and to foster the growth of politicized unofficial armed forces (and thus to cooperate in foiling Allied efforts at civilian disarmament).

All this Dixon makes clear, the speculative contention noted above notwithstanding, in a book that

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contains no surprises but much detail based on impressively solid research.

C. EARL EDMONDSON
Davidson College

KAREL KAPLAN. *Die politischen Prozesse in der Tschechoslowakei 1948–1954.* (Veröffentlichungen des Collegium Carolinum, number 48.) Munich: R. Oldenbourg. 1986. Pp. 228.

A former Communist party official, Karel Kaplan, has produced an extremely informative account of the political trials that took place in Communist Czechoslovakia from 1948 to 1954. For his study Kaplan has surveyed many unique source materials, which he consulted during the 1960s in the secret archives of the Communist party of Czechoslovakia and other state authorities. His narrative is largely based on the wealth of primary sources that he took with him when he left the country in 1976.

Since we live with the consequences of wartime decisions and since every year the number of those who had nothing to do with the atrocities of the early Communist era increases, there is a growing tendency to trivialize the Communist experience of 1948–54 in East Central Europe and to forget the ghastly nature of past brutalities. In Czechoslovakia alone some one hundred thousand people were sentenced by courts, innocent victims of political persecution.

The task of a historian is primarily to report on the past honestly and objectively. Kaplan does just that. In the first part of his book, he presents the political trials not as an aberration of Stalinism but rather as an indispensable part of the initial stage in the construction of a Communist system. The Prague regime used the trials chiefly as a device to help solve various social and political issues and to destroy existing social and political structures as well as national traditions. Ideology was mostly of secondary importance, used to prop up Soviet interests and to maintain and promote an all-embracing Communist dictatorship. The second part of the study covers the six years of the regime up to 1954. The first wave of trials in 1948 was still closely linked to the February takeover. Successive trials against the military, former political parties, the church, foreign powers, selected economists, and, ultimately, top Communist officials themselves marked the main steps.

Kaplan is at his best when he details factual descriptions. One could take exception to his inclusion of Nazi collaborators in the postwar trials. There are moments when one wishes for a more probing analysis, even though the study makes no pretense to being a work of deep scholarship or perception. His largest single contribution is his penetrating discussion of the trials as key elements in the Communist mechanism of power. Among the lists compiled by the author, those detailing the individual trials will prove of future value. The volume will take a well-deserved place in the literature dealing with the inner workings of Communist regimes.

RADOMIR V. LUZA
Tulane University

VLADIMIR VOLKOFF. *Vladimir the Russian Viking.* Woodstock, N.Y.: Overlook. 1985. Pp. xxiv, 384.

In a marvelously lively fashion this work portrays the development of Prince Vladimir, the Christianizer of Rus', from an excited child in his grandmother's palace in a Kiev under Pecheneg attack to a dying ruler exhorting his children to princely and Christian deeds (in words the author borrows from the *Testament* of Vladimir's great-grandson, Vladimir Monomakh). In between Vladimir Volkoff attempts to re-create the prince's life down to his campaigns and conversations. The book reads like a novel with resonances from various sources for the history of Kievan Rus', and, despite the Library of Congress call number assigned it and the author's protestations to the contrary, it is better as a novel than as history.

The work's major shortcoming as history results from Volkoff's preference for telling a good story rather than investigating the historical problems surrounding the life of the first Christian ruler of Rus'. The author has obviously read the basic sources for the Kievan period, but, rather than evaluating them critically and studying the scholarly literature they have evoked, he mines the material, willy-nilly, for anecdotes. Apparently because they tell good stories, the author draws on the "unreliable" *Joachim Chronicle*, as well as on Icelandic sagas and Russian folk epics (*byliny*), with little apparent attention to the reliability of their information. To describe Vladimir's bureaucracy, Volkoff projects back two generations information from the *Pravda of Iaroslav's Sons* on officials in charge of princely estates, with no hint as to the massive modern scholarship on the question of the very existence of private princely estates in the Kievan realm. Is a sixteenth-century letter of a patriarch of Constantinople to the Russian tsar on whom he depends for alms an appropriate source for the "imperial" regalia supposedly worn by Vladimir I in the tenth century? Perhaps it might be, if the thesis were argued cogently, but rarely does the author argue his case; he simply presents his material. More often than not, the basic material in the book is

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correct, but that is not enough for good history; readers must be able to depend on it (and this book has a significant number of factual errors) or be able to check it themselves in the sources and scholarship (and the endnotes are not sufficient to allow this).

This book is not successful as "the first complete biography of the extraordinary prince who founded the Russian state and converted his people to Christianity," as the dust cover would have it, but it does capture some of the lusty spirit of the tenth-century Kievan court much as a Monty Python film might do.

GEORGE P. MAJESKA
University of Maryland

CHARLES J. HALPERIN. *The Tatar Yoke*. Columbus, Ohio: Slavica. 1986. Pp. 231. $15.95.

Basing his work on original insights formulated by Michael Cherniavsky, Charles J. Halperin has become preeminent among American authorities on the Mongol-Tatar period in Russian history. This is illustrated by his more than twenty articles on the topic over approximately the past ten years and, most strikingly, by the appearance of his *Russia and the Golden Horde: The Mongol Impact on Medieval Russian History* in 1985 and of the present volume.

Perhaps moved by criticism that the bibliography of *Russia and the Golden Horde* was a trifle skimpy and one-sided, this time Halperin includes a thorough list of the Soviet literature (mainly historical and textological monographs) on the subject, which he often comments on in the notes. Western scholars concerned with these issues are not neglected.

Halperin has developed a provocative theory and has not been afraid to take on the prerevolutionary, Soviet, and other historical establishments concerning a theme over which passions understandably run high. His thesis is that Muscovite "bookmen" of the Mongol-Tatar period consciously refused to admit or discuss the fact that Russia had been conquered by and was obliged to remain subservient to Mongol-Tatar suzerains for well over two centuries. This does not mean their writings contain no information pointing in the opposite direction, but they were motivated by a hostility toward the Tatars that was purely religious in origin. Modern historiography in a secular age of nationalism has been unable to understand or cope with such an attitude.

To demonstrate his contention, in five central chapters that form the core of the book Halperin relates the content and discusses the provenance and language of principal writings of the time. He investigates the chronicles' portrayal of the Mongols' initial foray into Russia under Batu, the "martyrdom" of Prince Mikhail of Chernigov, the 1327 uprising in Tver', the Battle of Kulikovo, and the civil war in Vasilii II's time. He analyzes the Lives of Aleksandr Nevskii, the five sermons of Serapion of Vladimir, the story of *Baskak* Akhmed of Kursk, the *Skazanie o Mamaevom poboishche*, the *Zadonshchina*, and Vassian Rylo's "Epistle to the Ugra," to name but a few. He frequently cites examples from the sources to show that Russian leaders not only acknowledged but respected the legitimacy of a Tatar ruler descended directly from the house of Chinggis-khan.

Old favorites appear in a fresh light. Fascinating insights abound: no source here portrays Dmitrii Donskoi and his boyars and prelates exulting that they have at last freed Russia from a Tatar yoke; Halperin convincingly explodes the stubborn myth that the roots of Soviet expansionism should be sought in the "doctrine" of Moscow the Third Rome.

One might raise a few cavils. Halperin regularly writes about "Russia" as opposed to the "Golden Horde." It would surely have strengthened his position if he had further emphasized the lack of unity among the Russian princes during the Mongol-Tatar period; even at the end of Ivan III's reign there were still appanage rulers capable of offering resistance to Moscow. The religious establishment was the exclusive custodian of the written word during this epoch. Perhaps it might be possible to invert the *argumentum ex silentio* by saying that others in other walks of life in Muscovy may have viewed the Mongol-Tatars differently, in more secular terms. But these are quibbles in no way detracting from the book's intriguing thesis or the way in which Halperin has used the original sources to prove his case. An index would have been helpful.

HUGH F. GRAHAM
California State College, Bakersfield

J. L. BLACK. *G. F. Müller and the Imperial Russian Academy*. Kingston: McGill-Queens University Press. 1986. Pp. xi, 290. $34.00.

Born in 1705 in Herford, Westphalia, Gerhard Friedrich Müller arrived in Russia in 1725 with the sole purpose of learning the Russian language and becoming a Russian historian. He immediately became associated with the newly founded St. Petersburg Academy of Sciences, at first as a graduate student and after 1731 as a professor of history. Actively engaged in academic affairs, both scholarly and administrative, Müller played an important role in transforming the fledgling academy into a dynamic and internationally respected institution. Between 1754 and 1765 he occupied the powerful position of secretary of the academy. He authored the first history of the academy. As a

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member of the Second Siberian Expedition in 1733–43, he undertook the task of gathering archival material, describing native populations, and making geographical observations. Müller was particularly noted for his monumental historical study of Siberia. The first historian in Russia to establish the practice of subjecting documentary sources to critical appraisal, he was also a pioneer in organizing national archives. On his initiative, a series of studies by contemporary Russian historians appeared in print. In the middle of the century, Müller, inspired by the spirit of the time, initiated and edited *Monthly Compositions*, a journal intended to spread useful knowledge by presenting it in a language comprehensible to nonscholars. This journal marked an important step on the path to the modernization of Russian thought.

Despite the vast proportions of his achievement, Müller was one of the more controversial persons in the community of Russian historians. His endorsement of the Norman theory made him a target of bitter attacks by M. V. Lomonosov and S. P. Krasheninnikov, the ranking Russian members of the academy. The bizarre complexity of his ties with the "German Party" in the academy and the shifting and tangled web of his personal alliances make him a particularly difficult topic for historical inquiry.

J. L. Black's thoughtful and informative volume is the first major study of the vast spectrum of Müller's activities. In its coverage of the major phases of Müller's academic pursuits and personal entanglements, it combines a rich presentation of pertinent details with a keen concern with broader trends in the history of Russian culture. The running comments on modern—particularly Soviet—interpretations of Müller's historical stature give Black's study a valuable extra dimension. They show the persistence of a nationalist bias standing in the way of a more objective appraisal—and recognition—of Müller's formidable role in the intellectual culture of eighteenth-century Russia.

Black makes frequent references to the Russian Academy where he means the St. Petersburg Academy of Sciences or the Imperial St. Petersburg Academy of Sciences. The Russian Academy is a completely different institution, founded by Catherine II in 1783 to work on the improvement of the Russian language.

Black's study is a major contribution to the history of Russian culture. It is not only a perceptive biography of an eminent historian but also a rich source of information on the growing pains of the St. Petersburg Academy of Sciences during the initial decades of its existence.

ALEXANDER VUCINICH
Sea Ranch, California

MARY STUART. *Aristocrat-Librarian in Service to the Tsar: Aleksei Nikolaevich Olenin and the Imperial Public Library*. (East European Monographs, number 211.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York, 1986. Pp. ix, 245. $30.00.

This book is in part a history of the Russian Imperial Public Library to the middle of the nineteenth century and in part a study of A. N. Olenin (1763–1843) as a "principal actor in Russian intellectual, cultural, and political life" (p. ii). It is divided into ten chapters of quite disparate lengths (from eight to twenty-two pages). Three of these deal with the history of the library (2, 5, 9) per se; three others cover the role of Olenin in Russian culture generally (3, 7, 8); and in the remaining sections both his personal career and his contribution to the library are studied. The book is well documented and has a long and exhaustive bibliography, forty-six illustrations, and an index.

The period in which Olenin lived was a vital one in imperial Russian intellectual, political, diplomatic, and social history, for it encompassed the reigns of Catherine II, Paul I, Alexander II, and the early years of Nicholas I—each of whom had widely diverging ambitions for Russia. The French revolution, the Napoleonic wars, and, for Russia specifically, the Decembrist movement were all both symptom and cause of very dynamic crosscurrents in society. Mary Stuart's book adds to and complements other recent studies on G. F. Müller and S. S. Uvarov—who overlapped this period chronologically at opposite ends.

There is a lot of information in this book on both Olenin and the library, but the reader will wonder about the apparent skimpiness in some of the sections. I will cite but a few examples. The history of the academy gymnasium, 1764–1840, is dealt with in one paragraph (p. 100)—to what purpose? In several places Olenin's personal closeness to N. M. Karamzin is stressed (for example, pp. 58, 118), but this fact is never expanded on even though their views seem remarkably alike. Why did D. I. Khvostov refer to Olenin as a "member-parasite" in connection with his election to the Russian Academy in 1786? What was the nature of Olenin's "unpublished dictionary of ancient Russian military terms," or is this impossible to discover? The "sweeping reforms" that Olenin is said to have exacted at the Academy of Fine Arts are not delineated very thoroughly.

The sections on the National Library show the author at her best. They are precise, to the point, and informative. This is especially true of the later years of the library, where "users" are also scrutinized. Useful too are materials that relate Olenin to the activities of Uvarov, V. S. Sopikov, I. A. Krylov,

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N. I. Gnedich, N. I. Grech, and others. Among other things, these references corroborate very well what we already know and assume about the "buddy system" of the period.

J. L. BLACK
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INGEBORG FLEISCHHAUER. *Die Deutschen im Zarenreich: Zwei Jahrhunderte deutsch-russische Kulturgemeinschaft.* Stuttgart: Deutsche. 1986. Pp. 670. DM 58.

INGEBORG FLEISCHHAUER and BENJAMIN PINKUS. *The Soviet Germans: Past and Present.* Foreword by EDITH ROGOVIN FRANKEL. New York: St. Martin's, with the cooperation of the Marjorie Mayrock Center for Soviet and East European Research at the Hebrew University, Jerusalem. 1986. Pp. xi, 185. $29.95.

These two books provide a reliable and scholarly survey of the history of Germans and their descendants in Russia. Although Germans had been in close contact with Russians since the days of Kievan Rus', it was only in the imperial period that a large number became Russian subjects and agents of military, administrative, and economic change. With the help of Baltic and other Germans, Peter the Great and his successors hoped to transform Russia into a modern state in the Western sense. Thus, many German newcomers and their descendants received preferential treatment in the imperial system, the professions, and the military; others were given most desirable land for cultivation; and all were allowed, if they so desired, to preserve their linguistic, cultural, and religious traditions. Their very success and privileged status, however, increasingly brought them into conflict with native Russians, which culminated in Stalin's destructive measures during World War II.

Ingeborg Fleischhauer's work on Germans in tsarist Russia aims to clarify "questions of historical continuity" (p. 11). Thirteen fact-filled, well-structured chapters feature the contexts, size, and direction of German migratory moves into territories under Russian control. The author analyzes Russian governmental measures that remained remarkably consistent within general historical change. She ably chronicles efforts of individuals and communities of German origin to achieve success, in conformity with the general interests of the Russian state but without giving up their own ethnic tradition and privileged status. Her book has many merits. The style is exemplary, the chapters are lucidly constructed, and rich detail is marvelously balanced with perceptive generalization. The study is derived from a wide reading of the secondary literature in English, French, German, and Russian and is solidly based on years of work in archives of Germany, the Soviet Union, and the United States.

Fleischhauer's description of the Russian world, however, relies too heavily on older and often outdated German-language popular or scholarly accounts that tend to exaggerate German influence on Russian affairs. The more recent scholarly literature on both Russian bureaucratic and Baltic German history suggests that the whims and caprices of Russia's rulers were far less important than the difficulties resulting from efforts to transform a backward society consisting mainly of serfs or of state peasants and townspeople bound to their tax-paying commune into a well-regulated "police" state. In the face of these difficulties Russian Germans were as helpless as the Russians and had to adjust to, and interact with, an intractable Russian reality. Thus, the Volga Germans as cultivators of the soil were not very different from the neighboring Russian state peasants; they were hardly model farmers and pioneers of agricultural modernization, and they shared the attachment of local Russians to the highly inefficient repartitional commune. Many more Germans, furthermore, opted for Russian nationality than Fleischhauer seems willing to admit; she pays insufficient attention to the results of linguistic adaptation, acculturation, and assimilation among socially mobile Germans outside the agricultural settlements, the Baltic provinces, and the well-developed German community of Petersburg.

The book on the Soviet Germans provides a sort of epilogue to the previous work. It contains five "reshaped and considerably expanded" papers, originally presented at a conference held in Jerusalem in 1981, to offer "a new synthesis" on the history and sociology of Soviet Germans (p. v). Fleischhauer presents "a reappraisal" of the Germans in tsarist Russia, a convenient and dense summary of her extended study in German. Her essays "'Operation Barbarossa' and the Deportation" and "The Ethnic Germans under Nazi Rule" reveal how "the entire Russian German community paid the price for Hitler's eastward expansion with permanent loss of their historic home" (p. 102). Benjamin Pinkus's informative and balanced two chapters offer a sociological portrait of Soviet Germans from the October revolution to the present, whereas Edith Rogovin Frankel's introduction and epilogue add valuable general perspectives. This concise, interpretively balanced, and sensitive survey with rich, if not fully up-to-date, bibliographical detail shows how hundreds of thousands of mainly apolitical people with strong ethnic loyalties were devastated by Stalin's brutal retaliation against German expansionist and racist designs. The suffering inflicted was immense, relief for survivors years in coming, and rehabilitation remained woefully incomplete. If Fleischhauer's book evokes admiration for Russian

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magnanimity and Russian-German vitality, the chronicle of the Soviet Germans creates deep sadness in view of the tragic fate that befell a largely loyal and innocent people in the vortex of hegemonic struggles that culminated in two world wars. Post-1950 events, however, give hope that a new era of possibly mutual engagement may have dawned.

EDWARD C. THADEN and
LEO SCHELBERT
University of Illinois, Chicago

B. N. MIRONOV. *Khlebnye tseny v Rossii za dva stoletiia (XVIII–XIX vv.)* [Bread Prices in Russia from the Eighteenth through the Nineteenth Centuries]. Leningrad: Nauka. 1985. Pp. 300. 2 r. 50 k.

This book by the Soviet economic historian B. N. Mironov studies the history of grain prices in Russia from the eighteenth century to World War I. The book is valuable for two reasons. First, it provides a vast data base of Russian grain prices by region and by year from 1708 to 1915. This data base can be used by other scholars to study trends in grain prices by cross-section and time series. Second, it is an illustration of the state of quantitative economic history as practiced in the Soviet Union. Mironov seeks to explain the extraordinary increase in Russian grain prices between 1707 and 1915 (at a time when such prices were relatively stable in Western Europe) and to explain the sources of regional differentials in grain prices during different subperiods. Mironov applies standard supply and demand analysis to set up models of grain-price determination, which he then tests using multiple regression analysis. He finds that the intertemporal rise in grain prices was due to increases in the money supply, foreign demand factors, and supply factors. Mironov finds that regional price differentials resulted from regional supply and demand factors, locational factors, and port prices.

This study shows that Soviet economic historians have made substantial progress in the area of quantitative economic history. My one criticism is that Mironov sets up his models in a fairly *ad hoc* manner. The models appear to include empirical variables that may measure the same supply or demand factor. For example, Mironov includes both the physical quantities of exported grains and export prices in his regressions, whereas it might have been better to include only export prices. This criticism should not detract from the valuable contribution that this book makes to our understanding of Russian grain prices.

PAUL R. GREGORY
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JOHN DOYLE KLIER. *Russia Gathers Her Jews: The Origins of the "Jewish Questions" in Russia, 1772–1825*. Dekalb: Northern Illinois University Press. 1986. Pp. xxiv, 236.

In his oddly mistitled book John Doyle Klier seeks to answer three questions: what was the Jewish question, how did the Russians become aware of it, and how did they propose to remedy it? His answers are clearly stated and supported by the relevant published sources—unpublished archives have either disappeared (papers of the Jewish Committee fell victim to fire in the nineteenth century) or were not accessible to him in the Soviet Union. In answering the questions, Klier paints a fair picture of the relationship between the Jewish minority and the majority populations, on the one hand, and the imperial administration, on the other. He tries to see both the Jews' social, economic, and cultural predicaments and the problems faced by the local populations and administration.

The Jewish question arose as a consequence of the annexation by the Russian empire of formerly Polish and Lithuanian territories inhabited by Jews and the government's utter unpreparedness for dealing with them. Before the annexation the Jews had been a constituent part of the society and economy of these territories—their condition was neither good nor secure, but it had the advantages of longstanding practice and tradition. The Jews had developed an institution of fiscal and police self-administration, the *kahals*, which were controlled economically by the well-to-do and policed culturally by a traditional rabbinate. The Russians became aware of the Jewish question for two reasons: first, through the social tensions and economic conflicts between Jews and the local "native" population and, second, through the barrier that Jewish religious and cultural particularism seemed to present to a policy of absorption into a uniform imperial administrative and social system.

In this situation, the several groups living in the newly annexed territories—Polish landlords, magnates, peasants, church, and the Jews with their institutions—tried to gain as many advantages with the help of the government as possible. For example, Polish landlords used the Jewish question to continue their exploitation of the peasantry while shifting all the blame onto the Jews; the *kahals* endeavored to scotch *Haskala*-inspired modernizing cultural trends by playing on the government's fear of social and economic destabilization.

Finally, the complexities of the Jewish question proved beyond the capacities of the imperial government in that period. The enlightened approaches that were suggested (perhaps on the false premises of prior religious change) could not be implemented because of the resistance they met on

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the part of all concerned; it was easier, and less costly, to maintain the status quo. True, the status quo was modified, but in such a way as to placate the local elites; as a result, the Jews were pushed into an even tighter corner and deprived of whatever latitude they may have had to modernize their economic and cultural conditions. Justifications of the pessimistic anti-Semitism of leading officials (such as G. R. Derzhavin) and the impossibility of a humane and progressive solution to the Jewish question proved a self-fulfilling prophecy.

Klier tells the story of governmental decision and indecision well and with much useful detail. He marshals the evidence clearly and offers balanced judgments. But the story is told from the outside, as it were. One does not gain the impression that Klier is really at home in the situation. The predicaments of the Jews and their ways are treated from the point of view of the leaders of the German-type enlightened modernizers; peasant-noble and imperial-local tensions in Poland-Lithuania are seen in simple liberal and St. Petersburg–oriented perspectives; and the quandaries of the imperial government, although best illuminated and understood, also suffer from a lack of depth and too often are viewed in the light of later developments. These criticisms should not detract from the intrinsic worth of the book as the only detailed and balanced account of the first act in the sad drama of Jewish-Russian relations since the two were thrown together by the Partitions of Poland.

MARC RAEFF
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S. B. OKUN'. *Dekabrist M. S. Lunin* [The Decembrist M. S. Lunin]. 2d ed. Leningrad: Leningrad University Press. 1985. Pp. 278. 1 r. 50 k.

This work by S. B. Okun' is the second "corrected" edition of a book first published in small quantity (2,000 copies) in 1962. A specialist on the early nineteenth century, Okun' brings us here a biography of the most fascinating of the Decembrist revolutionaries. Nobleman, cavalier, Catholic, and conspirator, M. S. Lunin once daringly challenged the tsar's brother to a duel, proposed regicide by armed band as early as 1816 (although he later changed his mind), defied authorities (unlike his comrades) by adroitly avoiding any betrayal of the revolutionaries despite imprisonment and interrogation, and became the only surviving Decembrist to continue antigovernmental writing and crusading for all of his life.

Okun' has diligently combed through archival and published sources to reveal any mention of Lunin in all possible contexts. Indeed, this biography often reverts to historiography as the author discusses views of contemporaries as well as later scholars on the minutiae of Lunin's viewpoints and activities. Nevertheless, because of conspiratorial secrecy and the destruction of documents by Lunin and authorities alike, many conclusions may never be thoroughly verified. The thesis that Lunin was closer to P. I. Pestel' (Southern Society) than to his friend and cousin Nikita Murav'ev (Northern Society) in regard to the important matters of regicide and republicanism remains tenuous. Lunin seems to have changed his mind on regicide and never committed himself to republicanism—he saw presidents and monarchs as equally acceptable executives as long as they were carefully controlled by constitutional law. Lunin agreed with Pestel' on the evils of serfdom, but on the question of liberating serfs with or without land his stand was less certain. Even more than Pestel', he saw the Poles as friends and allies in revolutionary endeavor. Unfortunately, Okun' is no more successful than the original investigators in documenting any ties between Lunin and Pestel' during the years immediately preceding the Decembrist uprising. Because authorities immediately destroyed much evidence, the exact cause of Lunin's death (in a notorious political prison) is likely to remain a mystery.

Amid meticulous reporting of interrogations and paragraph-by-paragraph analysis of Lunin's post-December writings, one theme is missing here. We see only the political man, not the ardent (perhaps even fanatical) man of religion that Lunin actually became. This dominating part of his life was more thoroughly reported by Glynn Barratt. In other ways, particularly regarding Lunin's actual involvement in the Decembrist conspiracy, Barratt leaned very heavily on Okun' for information. Between the two volumes (both flawed or incomplete) we now have a great deal of information on Lunin.

DEBORAH HARDY
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STEVEN L. HOCH. *Serfdom and Social Control in Russia: Petrovskoe; A Village in Tambov*. Chicago: University of Chicago Press. 1986. Pp. x, 220. $25.00.

The single-community monograph is still an underrepresented, not to say unrepresented, genre among social historians of prereform Russia. For that reason alone Steven L. Hoch's study of the serf estate Petrovskoe (and its largest village, also called Petrovskoe) in Tambov province in the first half of the nineteenth century is a welcome addition to the English-language literature on the Russian peasantry. To have overcome serious restrictions on research—a stay limited to nine months in the Soviet archives, the need to copy census information by hand—and to have produced a book-length analysis

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is admirable enough. But Hoch must be credited with two other accomplishments. His research has corroborated Peter Czap's earlier demonstrations in a series of studies of Mishino estate that Russian imperial sources stand as a barely tapped gold mine for investigators of peasant life. And, not least, Hoch has shown how much more generally satisfying it is to research the Russian variant of serfdom by looking at the "system" from the vantage point of its operation below the general legal categories.

Serfs were property of a sort, and their owners (in the case of both Hoch's Petrovskoe and Czap's Mishino, the noble Gagarin family) had a watchful eye kept over them. This produced a diverse legacy: reports from bailiffs, manuals of discipline, accounts of economic transactions, and, most notably, household inventories of persons and property. Some of these sources are quantifiable; others yield anecdotal evidence about serfs' actual behavior. Though very rich, the evidence does not permit full scrutiny of all domains of the socioeconomic experience and, because of its diversity, requires a theoretical framework for coherence. Guided by the writings of Eric Wolf ("Peasants are by definition exploited" [p. 1]) and others concerned with the unequal distribution of power, Hoch portrays the peasant-serfs of Petrovskoe as living under a harsh system in which key members—the estate owners, the bailiffs, the heads of peasant households ("patriarchs")—collaborated to extract work and obedience from their subordinates. In separate chapters Hoch explores the material components of the system (including the practice of land partition), considers the demographic features of the population, reviews the meshing interests of the bailiffs and the patriarchs, and then, in two extremely interesting sections, discusses the ways the peasant commune (*mir*) exercised control and meted out punishments. Each of these topics requires a somewhat different kind of analysis, ranging from descriptive statistics to inferences about the entire population from the experiences of individuals. In the end, the peasants of Petrovskoe emerge as able agriculturists, with levels of productivity roughly equivalent to those of Western peasants. But they also resisted the authorities and were prone to all manner of thievery and violence against each other and their superiors. In Petrovskoe peasant life, according to Hoch, was Hobbesian: "hostile, violent, vengeful, quarrelsome, fearful, and vituperative" (p. 189).

Hoch asserts that "the historian's task is not to generalize but to integrate" (pp. 2–3), and, on the whole, he puts forth a valiant effort of sticking to his maxim. Nonetheless, the book is full of generalizations, many rather more confident than most historians doing microlevel analysis could accept. By the last chapter the Petrovskoe peasants have become "the Russian peasants" (p. 187). "Love," it is asserted, "was not a factor in [peasant] marriage and was even thought to be harmful" (p. 102). Although it is quite proper in a study of "serfdom and social control" to focus on the domain of authority relations, to use only these relations as the basis for statements about "peasant life" in general is questionable. Hoch also persists, perhaps inadvertently, in slipping from discussions of serfdom into statements about slavery, as if those two modes of subordination were not different (pp. 11, 172, 179). Many of these problems, it would seem, arise from the fact that Hoch evidently does not expect much diversity in the tens of thousands of Russian and non-Russian villages of the empire: "Serf villages were not radically individualistic in resolving tensions and conflicts, responding to servitude, controlling access to productive resources, and establishing family patterns" (p. 3). The book suggests an impatience about "the big picture" that ill serves the subject at hand, even while establishing Hoch as one of the pioneers of this promising area of research.

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NIKOLAI S. LESKOV. *The Jews in Russia: Some Notes on the Jewish Question*. Translated by HAROLD KLASSEL SCHEFSKI. Princeton: Kingston Press. 1986. Pp. xli, 143.

In 1883 Baron Horace De Gunzburg commissioned the writer Nikolai S. Leskov to write a defense of Russian Jews to influence the decisions of the Pahlen Commission, a group established by Alexander III to examine the Jewish question. Leskov was a perfect choice for this task: he not only was known for his tolerant religious ideas but often depicted Jews in a positive and sympathetic light, free from the negative stereotypes that characterized much of nineteenth-century literature. The result was *The Jews in Russia: Some Notes on the Jewish Question*, a trenchant plea for religious tolerance and equal rights for Russian Jews.

Leskov attacked the traditional fears and attitudes that formed the basis of anti-Semitism in Russia by demonstrating that Jews posed no religious, economic, social, or moral threat to gentiles. He skillfully undermined arguments concerning supposed Jewish economic exploitation, lack of patriotism, military and agricultural incompetence, failure to assimilate, and moral and spiritual turpitude and convincingly argued that Jews were not solely responsible for their failure to act more in accordance with the wishes and dictates of the gentile majority. For example, Jews should not be held accountable for Russia's drinking problem since Russians took up drink long before Jews came to operate many of the taverns in the Pale of Settlement. Given popu-

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lation pressures and limited economic opportunities in the Pale of Settlement, Jews should not be condemned for pursuing economic ventures that satisfied consumer demand. Similarly, members of other ethnic minorities, notably Greeks and Germans, exploited the Russian peasantry but did not find themselves the object of special discriminatory legislation.

Harold Klassel Schefski's annotated translation of Leskov's book is a timely and welcome addition to the growing literature in English on the Jewish question in late imperial Russia. Schefski has also written an introduction that provides a schematic survey of Russian Jewry in the nineteenth century and discusses the book in the context of Leskov's other works that have Jews as protagonists. It is indeed a shame that the editors at Kingston Press did not perform their job more conscientiously for the text has typographical errors that mar an otherwise valuable source on nineteenth-century Russian perceptions, attitudes, and prejudices toward Jews and other non-Russian nationalities.

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NEAR EAST

DAVID S. POWERS. *Studies in Qur'an and* Ḥadīth: *The Formation of the Islamic Law of Inheritance*. Berkeley and Los Angeles: University of California Press. 1986. Pp. xiii, 263. $30.00.

The basic premise of this book is that, for many centuries, scholars have erroneously interpreted both the form and intended content of the Islamic law of inheritance (*'Ilm al Fara'id*). This thesis is based on what the author perceives to be a "secondary stage of development" (or actually several stages) in the history of Islamic jurisprudence, or *Fiqh*. Such stages of development consciously altered "proto-Islamic" provisions for inheritance by pursuing alternative interpretations of original Koranic texts. Because David S. Powers's method of reconstruction of assumed proto-Islamic intentions involves extensive philological investigation of Arabic terminology, some sections of the work will be of particular, but not sole, interest to Arabists.

Despite the book's focus on a specific area of Islamic law, the author takes considerable time to place his methodology in the broader context of "traditional" writings on the subject of *Fiqh*. Here, Joseph Schacht's widely recognized and respected opus *The Origins of Muhammadan Jurisprudence* (1950) receives very critical treatment. Schacht's approach in *Origins* posited the compilation, generations after the Prophet's death, of a body of "living tradition." This was developed by scholars who carried out systematic comparison of observable practice with Koranic norms and developed a "back projection" of legal doctrine. An alternative approach, originally posited by the late nineteenth-century French orientalist William Marcais, emphasized "superimposition" or selection by Muhammad of new principles, reflected in the Koran, to replace preexisting Arabian norms. In the specific area of inheritance, this approach developed a number of (mainly philological) means to show that Islamic law consciously offered improvements over inequities in Arabian tribal customs.

Powers's concern is not only to challenge these two schools of thought on the practical origins of Islamic law but also to demonstrate how a search for proto-Islamic provisions should change views of what Islamic law on succession actually intended to do. The resulting analysis suggests that "true" (proto-Islamic) law "not only allowed for full testate succession," including the designation of a testamentary heir, but relied only marginally on the later famous "science of the shares," with its involved mathematical intricacies.

The second half of this book addresses the question of why, in the context of certain historical developments, Islamic jurisprudents favored certain types of reinterpretations of specific Koranic verses on succession. Some suggestions are direct, especially in cases in which motivation for later *hadith* variations can be assumed (questions relating to claims against the Prophet's intestate estates, caliphal succession, and so on). A bigger problem is posed by the existence of multiple cases, in the Koran itself, of apparent abrogations (*Naskh*). Overall, legalists would eventually try to identify and interpret more than five hundred such texts.

Since Powers is concerned with the assumed recognition of bequests in proto-Islamic legal practice, the extensive legal debate over its assumed abrogation provides an ideal framework for textual analysis of positions taken on inheritance at different stages in the evolution of Islamic *Fiqh* (from "early doctrine," ca. 650–799, to "classical doctrine" during the tenth to fifteenth centuries).

Finally, one other methodological subfield of *Fiqh*, the study of *Asbab al Nazal*, or "circumstances of the revelation," receives similar detailed treatment as it affected, or tried to affect, interpretations of Koranic provisions regarding succession. By this point, however, readers—unless they are fully involved in this particular subfield of Islamic law—will probably be weary of repetition of the basic "heirs to shares" theme underlying interpretive nuances in the Islamic sources that are quoted.

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JOEL L. KRAEMER. *Humanism in the Renaissance of Islam: The Cultural Revival during the Buyid Age.* (Studies in Islamic Culture and History.) Leiden, The Netherlands: E. J. Brill. 1986. Pp. ix, 329.

In this book, Joel L. Kraemer introduces us to the circles of powerful, erudite, gracious, and learned philosophers, courtiers, skeptics, and free-thinkers—the milieu of the Baghdadi political and cultural elites who flourished in the wake of the breakup of the 'Abbasid empire and the political tumults that followed. This book is organized into three sections. Part 1 gives a résumé of the politics and social issues of the period. It discusses the rise of the Buyid Dynasty, its religious and cultural policies, its relations with the 'Abbasid Caliphate, Muslim religious and theological schools in Baghdad, the position of non-Muslim minorities, and other social and political issues. Based primarily on secondary materials, this does not represent an original contribution but is a useful and densely detailed summary of the period.

Part 2 is the principal contribution of the book. Here Kraemer introduces us to the Baghdadi philosophic and scholarly circles of the tenth century. Based primarily on the memoirs of Abu Hayyan Al-Tawhidi, a scribe, courtier, and intellectual of the age, Kraemer gives brief biographies of the principal philosophic figures and their students and followers, describes their discussions and debates, and passes on the quarrels and gossip of the tenth century. His brief accounts of the writings and discussions of the schools bring alive the questions that moved them: love and strife, contemplation and action, science and religion. Two particularly valuable contributions are his summaries of the lively and extensive debate on the utility and validity of astrology and the discussion of friendship and the devotion of disciple to master. For these schools, "philosophy was considered a way of life, a pathway to perfection and happiness, and the master was regarded as a guide and savior who leads his followers along the path to their goal. The ideal philosopher guide is a sage devoted to the enhancement of life, to eternal life" (p. 147).

Part 3 gives us the biographies of six of the leading literati, courtiers, viziers, and princes of the time. These biographies portray the intellectual interests and the interplay of culture and power characteristic of the age.

The philosophic interests and the strong personalities of the era lead Kraemer to characterize them in his introduction, conclusion, and in occasional passages as spokesmen for humanism in an age of renaissance, but the analogy is not persuasive. The analysis that would validate the comparison is missing. Although giving a wealth of detailed information, Kraemer does not discuss the values and *mentalité* that would confirm the comparison, nor does he explore the cultural and social context that would likely disconfirm the comparison between Baghdad of the tenth century and the European Renaissance. The value of the book lies in its richly detailed biographical and literary information.

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LOIS BECK. *The Qashqa'i of Iran.* New Haven: Yale University Press. 1986. Pp. xvi, 384. $30.00.

The Qashqai confederacy was the second largest in Iran, with perhaps four hundred thousand people in five major and several minor tribes. Migrating past Shiraz between pastures near the Persian Gulf and south of Isfahan, they presented a security problem but were more controllable than tribes in remoter or frontier-straddling districts. They played a big part in the economy and politics of Fars and in fighting in both world wars (particularly in 1918 in league with the intrepid German agent Wilhelm Wassmuss), but not in Tehran politics as the Bakhtiaris did from 1909 to 1912. Mainly Turcophone (though mixed in origin), they form an ethnic minority. They are well known for their handicrafts and fine "Shiraz" or "Qashqai" carpets made by tribeswomen and women of villages owned by the Qashqai. They have received considerable attention from Western writers, particularly anthropologists.

The present work is comprehensive, covering history to 1953, anthropology (tribal structure and economy), and recent events to 1982. Lois Beck obtained her material during stays in 1963–64, 1969–71, 1977, and 1979, from histories, archives, and so on, and from Qashqai informants in Iran and in exile. The excellent black-and-white photographs are her own.

She explains how the confederacy took shape in the late eighteenth and nineteenth centuries when the state was too weak to govern and tax the tribes directly, whereas the tribes needed a joint organization to promote their interests. The state-conferred office of *ilkhani* (paramount chief) became hereditary in the Turcophone but originally nontribal Shahilu family. In the troubled early twentieth century, the *ilkhani* Isma'il Khan Sowlat ed-Dowleh raised the confederacy to its greatest strength. Later, the Qashqais were hard hit by Reza Shah's attempt to subdue and sedentarize Iran's tribes, particularly those once involved with foreign powers. This and the unnatural death of Isma'il Khan in prison prompted a Qashqai feud against the Pahlavi dynasty. The tribes enjoyed a resurgence in the years up to 1953, but, after making a show of force in

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favor of Mohammad Mosaddeq, Isma'il Khan's sons were exiled. The author, relying on Qashqai sources, is perhaps overly sympathetic in some of her judgments. Qashqai raiding, extortion, and gunrunning at times endangered trade, agriculture, and security. In general, however, her historical account is both full and fair.

The anthropological chapters are admirably clear and free of jargon. Regrettably, the carpet making is not discussed. The author notes that nomadic sheep breeders were normally better off than peasants but does not mention their vulnerability to natural calamities. Loss of pastures and flocks because of late frost was a factor in the tribal uprisings of 1963.

Under recent events, Beck discusses the land reform, which adversely affected nomads, and the drive for industrialization, which drew many Qashqais into urban employment. She also appraises the tent-school system set up with U.S. help by the Qashqai educator Mohammad Bahmanbegi. Iran's tribes played no part in the Islamic revolution, but most Qashqais were glad when the Pahlavi monarch fell. The exiled khans, after contacting Khomeini, returned from the United States and Germany, and one of them, Khosrow Khan, was elected to the Islamic republic's parliament; but in May 1980 he was accused of being a CIA agent on evidence purportedly obtained from a U.S. embassy hostage. For two years these khans and some followers held out in the mountains. Eventually Khosrow Khan was caught and publicly hanged. Here again Beck's comments are valuable.

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EMERITUS
University of Durham

NIKKI R. KEDDIE and ERIC HOOGLUND, editors. *The Iranian Revolution and the Islamic Republic*. (Contemporary Issues in the Middle East.) Rev. ed. Syracuse, N.Y.: Syracuse University Press. 1986. Pp. xi, 246. $14.95.

The Islamic Republic of Iran has existed for eight years as of this writing. The Iran-Contra arms sales scandals that badly damaged Ronald Reagan's administration from the autumn of 1986 demonstrated that, although Iran had slipped off American front pages, it had hardly become irrelevant to American foreign policy. It becomes important, therefore, for Americans to understand contemporary Iran, simply because a country of some 55 million inhabitants, with substantial petroleum reserves and long borders with the Soviet Union and the Persian Gulf, cannot be ignored by a world power.

The hostage crisis of 1979–81, however, has left much bitterness and many obstacles to understanding, both conceptual and practical. With no U.S. embassy in Tehran and no American academic institutes headquartered there, observation of the country by Americans has to be carried out from a distance, rendering mosque watching an even more arcane art than Kremlin watching. For these reasons, we still have only a handful of books that seriously treat Khomeini's Iran. The present volume edited by Nikki R. Keddie and Eric Hooglund is one highly successful such effort. Through its concentrated analysis of clerical politics, economics, and culture, it usefully supplements the insightful narrative accounts recently produced by Shaul Bakhash and Ruhollah Ramazani. The new edition has allowed the authors to revise and update to late 1985 or early 1986 papers that had been given at a 1982 conference and published in typescript.

Some of the more valuable insights in the volume come from chapters on the development of revolutionary Iran's economy by Setareh Karimi and Reinhold Loeffler. Karimi's verdict on the regime's economic performance is that it has been mixed. Among the successes has been keeping Iran out of debt. When one considers the plight of other countries in the region, such as Egypt, one must admit the wisdom of this financial independence. As Karimi notes, however, Iranians have paid a price for this independence insofar as capital formation has fallen. Despite the war, government expenditures have fallen somewhat in real terms (demonstrating how spendthrift were the shah's habits). Private consumption has decreased, falling at its lowest point to 20 percent of the 1977 level, but this decline was largely borne by the top 20 percent of the population. There has been a shift in favor of rural households, and on the whole consumption among the bottom 80 percent has remained stable or risen slightly. Gross national product declined in the first years of the revolution, although the period 1982–84 showed improvement. The main problem of revolutionary Iran's economy is the same as that encountered by the Pahlavis: overdependence on petroleum income, to the vagaries of which the gross national product is closely tied. As Karimi points out, a fall in oil prices such as that of 1986 can thus be disastrous.

Loeffler perceives a great government investment in rural development, although local engineers and officials complained of central mismanagement and of political repression. Although the 100 percent increases in the development budget of the first two years of the revolution have slowed, in 1985 Loeffler found an extraordinary amount of government spending in a southern province on school and office buildings, roads, clinics, water pipelines, and small-scale industry. He thinks these observations can be generalized to the rest of Iran. He notes that high inflation has hurt teachers and government

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employees and that low fixed prices for wheat have hurt medium farmers. On the other hand, handsome profits could be made from herding products, whose price has kept pace with inflation, and this sector has expanded. Pastoralists were again doing well out of animal husbandry, their traditional preserve. Peasants could supplement their income with wage labor in the towns and cities. Ominously for the future, family planning programs have been abandoned and the government now urges large families. Although Iran's population density is relatively low for the Middle East, its arable land forms a small part of its territory, and high fertility rates could easily produce overpopulation.

Shahrough Akhavi and Hooglund discuss the role of the Shi'ite Muslim clergy, or ulema, who now dominate Iran's legislature and government the way lawyers dominate those of the United States. Akhavi discusses the continuing split among the clergy over the issue of whether a single cleric should rule, as Khomeini maintains, or whether a collective of high ulema should generally supervise society, leaving government to the laity. Another cleavage derives from Khomeini's opposition to bureaucratization and his commitment, rather like that of Mao Tse-tung, to permanent revolution. So far ulema opposed to direct rule by a single cleric and to permanent revolution have been silenced, but Khomeini's death could open the issue again. (Hashemi Rafsanjani, with whom John Poindexter and Oliver North dealt, does not, incidentally, show up in the list of "moderates" but rather in that of hardline Khomeinists.) Hooglund finds revolutionary clergy in southwestern Iran to have overwhelmingly originated in rural families of modest means.

Ideology and cultural studies are a bit slighted in this volume, but Keddie shows that the Twelver Shi'ite branch of Islam from which Khomeini springs has not always been revolutionary, and Adele Ferdows discusses the contradictions in the thought of the prerevolutionary radical Muslim theorist Shari'ati on the issue of women in Muslim society.

A final section of the book deals with Soviet and U.S. diplomatic relations with revolutionary Iran. William Miller prophetically explores the possibilities of a new relationship between Iran and the United States. Muriel Atkin brings great expertise to her discussion of the Soviet perception of revolutionary Iran, from an early enthusiasm for the regime as anti-imperialist, to a dismay over the war between Iran and a Soviet ally, Iraq, and, finally, to a cooling of relations after the 1983 destruction of the Communist Tudeh party and increasing Iranian involvement in the Afghanistan issue. Richard Cottam lucidly argues that Reagan's administration is mistaken in perceiving all issues, including Iran, solely in terms of a Manichean struggle between a powerful, expansionist Soviet Union and the United States. Cottam deserves credit for hitting the nail on the head. This mindset contributed mightily to the Iran-Contra debacle.

The new edition of this book, despite its having been overtaken by a major development in Iranian-U.S. relations, remains a valuable and readable contribution to our understanding of contemporary Iran.

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WADE R. GORIA. *Sovereignty and Leadership in Lebanon, 1943–1976*. London: Ithaca; distributed by Humanities Press, Atlantic Highlands, N.J. 1985. Pp. 286.

An unexpected result of the Israeli invasion of Lebanon in 1982 has been an increased scholarly interest in that country. Wade R. Goria's book is one of a large number of volumes that have appeared in the last five years. The author, in this fine addition to our knowledge of contemporary Lebanon, argues that if there was any chance for the young and insecure country to survive, its leaders had to rule wisely. This was especially so for the Maronites who under the National Pact of 1943 were given the primary responsibility for governing. The author lays much of the blame for the failure of the political system on Camille Chamoun and Suleiman Frangieh. Chamoun was president from 1952 until 1958, and Frangieh from 1970 until 1976. As president, Chamoun violated the National Pact by seeking protection from the West and thus effectively isolated Lebanon from the remainder of the Arab world, especially its Nasserites. Chamoun is also criticized for being the leader of a Maronite rejectionist front in 1975–76 that could have pursued a compromise solution to the country's problems and thus have averted its present chaotic state. Frangieh comes in for equally scathing denunciation for acting more like a village feudal lord than the leader of a nation. Of all Lebanon's postindependence presidents, he was "least effective" (p. 246).

Interestingly, however, Goria saves his most serious indictment for Kamal Jumblat, the mystically inclined Druze leader. The author feels, much as do David Gilmour (*Lebanon, the Fractured Country* [1984]) and Helena Cobban (*The Making of Modern Lebanon* [1985]), that Jumblat wanted to overthrow the sectarian system that kept him from power because he was not a Maronite. The implication of Goria's argument is that Chamoun and Frangieh were acting defensively to protect a legitimate system, whereas the Druze chieftain attacked it and was therefore more responsible for what followed. To

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my mind, however, not putting some value on the merits of each position makes all positions equally valid. Certainly, the sectarian-based National Pact badly needed to be overhauled.

Goria is at his best in chapter 5, in which he traces the economic precursors to the civil war. Although these roots of the conflict are now forgotten, it is well for historians to be reminded of the lengths to which people will go to achieve their economic rights and the steps to which some governments will go to prevent their achievement. The governmental attacks on the striking workers of the Ghandour chocolate plant in November 1972, security forces' assaults on small tobacco farmers in January 1973, clashes between students and police in January and February 1973, and the infamous army attack on demonstrating fishermen in February 1973 are, to his credit, accurately portrayed by Goria.

There are some whom the author praises. Former president Fuad Chihab is one. Goria calls him stable, well read, and intellectual. Of course, Chihab, even among his supporters, is noted for his use of the secret service to control the populace. In light of what has happened recently in the country one can understand the yearnings of the author as he writes that Frangieh's doing away with the Deuxieme Bureau weakened the state (p. 248), which needed "strong coercive measures" in "the overall interest of Lebanese internal sovereignty" (p. 79). Still one is hard-pressed to see how placing a strong secret service in the hands of Lebanon's worst president could have helped.

One should not dwell too long on the negative for fear of giving the impression the book is weak. It is not. It is a fine study of the post–World War II political leadership of Lebanon. Goria's analysis of the major actors in recent Lebanese history is outstanding. This book is a good place to locate sketches of men such as Pierre Gemayel, Riad Solh, Rashid Karami, and Musa Sadr. Since the author asserts that the Maronite monastic orders have increased their sway on the Maronite community, I wish that he had included lesser figures such as Charbel Kassis. The same holds for the Shiite leader Nabih Berri.

A few rectifications: Nassib Matni was the editor of *al-Tilighraf* not *al-Tayar* (p. 42); Fath was founded in 1957 not 1967 (p. 91); and Linda Sadaka does not spell her name Sadka (p. 8). The correction of the spelling of Sadaka's name is especially important to the thousands of us who have wandered through the Jafet Memorial Library at the American University of Beirut and been helped by that marvelous woman.

WILLIAM W. HADDAD
California State University, Fullerton

AFRICA

A. D. ROBERTS, editor. *The Cambridge History of Africa*. Volume 7, *From 1905 to 1940*. New York: Cambridge University Press. 1986. Pp. xx, 1063. $85.00.

The *Cambridge History of Africa* is now complete in eight volumes, each of about one thousand pages, covering a period from the origins of the human species to 1975. This achievement—to one such as myself who began his graduate work in African history in the early 1950s, when academics still questioned whether Africa had a history at all—is a staggering index of the explosion of historical knowledge in this generation.

The present volume deals with the "classical" period of colonial rule in Africa. By 1905 the European partition of the continent was virtually over, although Morocco, Libya, and Ethiopia had yet to experience colonial rule and the German possessions would be repartitioned in the aftermath of World War I. The volume ends in 1940, a date seen as ushering in the new era of nationalism and opposition to colonial rule.

A. D. Roberts's introduction resists the implication that this makes the period "a pause between power struggles, significant mainly as a prelude to Africa's coming of age." Instead, he offers "more arresting perspectives" by emphasizing economic, social, and cultural perspectives "as important and interesting as any in the history of Africa" (p. 1). The contributors follow this lead, emphasizing economic themes and the history of ideas. The first five chapters are pan-African in perspective. Andrew Roberts surveys colonial policies and attitudes in "The Imperial Mind," and Christopher Wrigley, in what is clearly the outstanding chapter in the volume, masterly accomplishes the nearly impossible task of outlining the main trends of economic history on the continent. Chapters follow on Christianity, by Richard Gray, and on Islam, by C. C. Stewart, and Roberts contributes an enterprising chapter, "African Cross-Currents," which examines African reactions to colonial rule as a series of ideas emerging from the impact of capitalism and labor migration, the pressures of governments, and missionary education.

The remaining chapters deal with individual regions of Africa, some defined geographically, some grouped according to the colonial power. Here, the approach is more textbookish; most of the authors produce intensive distillations of recent monographic and article literature (which fanatics can follow up in an elaborate and magnificent 207-page bibliographical section of the type that has become a hallmark of the *Cambridge History of Africa*). Two of the regional chapters stand out. B. Jewsiewicki presents the best short account of Belgian Africa yet to appear in English. Richard Caulk's chapter on Ethi-

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opia and the Horn must now become the standard introduction to that area in the period that saw the only independent precolonial African empire struggle toward reform and "modernization," be conquered by fascist invasion, and be liberated by a British imperial military action in support of African resistance, which broke all the conventions of European racial solidarity that had held firm since the partition.

Writing the history of an entire continent presents enormous problems. Perhaps there is European history, or even American (as distinct from U.S.) history. But a continental history of Asia or Africa is more difficult to grasp. Chapters on North Africa in this volume fit uneasily into seemingly arbitrary juxtaposition of Middle Eastern and African historical traditions. The unifying themes, more often than not, seem rooted in the policies of colonial powers ruling in both Mediterranean and sub-Saharan areas, rather than in any "Africanness" common to both zones.

The British imperial apparatus sounded its last bugles and folded its flags in Africa some twenty-five years ago, rapidly, though more reluctantly, followed by France and Belgium, tardily by the Portuguese. Yet, even this volume makes clear that historians are still too close to these events to view them dispassionately, as we might view Roman imperialism in Britain or Gaul. Despite the editor's hopes, the impression left by the volume is that of a "pause" between conquest and nationalism. Few of the authors appear to see any integrity in the period, and most of their chapters lack conclusions, except for somewhat mystical assertions that, in 1940, nationalism was in bud and waiting to flower. All the regional chapters assume that African nationalism was a polar opposite of imperialism, rather than its direct offspring and heir.

Of the fifteen authors of this volume, eight are British, four American, two French, and one Canadian: none are African. This surely demanded some comment from the editor. One suspects that Africans were indeed asked to contribute and that their absence reflects the sad decline of library resources, facilities for research, and time for reflective writing available to our colleagues in Africa's hard-pressed universities.

JOHN FLINT
Dalhousie University

TOUFIK AYADI. *Mouvement réformiste et mouvements populaires à Tunis, 1906–1912*. (Faculté des Lettres et Sciences Humaines de Tunis, number 30.) Tunis: Publications de l'Université de Tunis. 1986. Pp. 300.

Tunis and its Muslim population played an active role in anticolonial movements in North Africa before and just after the turn of the century. For this reason Toufik Ayadi's study is very welcome; it complements the work of Jean Ganiage on the beginnings of the French protectorate there. During the six-year period discussed by Ayadi (1906–12), a number of episodes took place that clearly pointed the way to Tunisian independence.

At Tunis a simmering discontent among the "autochtones" was based on their attempts to obtain education, to raise their living standard to that of the French occupiers, to stop settler acquisition of land at derisory prices, and to try to halt daily incidents of racism and intolerance. Generally, the "Young Tunisian" movement (modeled on the Young Turks) had complete popular support; it was equally solidly opposed by the colon element headed by Victor de Carnières. The European settlers also did their best to derail Tunisian Muslim schools such as the Sadiqiya, the Khalduniya, even the Zaytuna Mosque school and to put pressure on Muslims wherever possible. Ayadi gives a good account of the local reaction to these moves, which ranged, as might be expected, from partial acceptance of France as a beneficial colonial power to openly anti-French and pan-Islamic manifestations.

Undoubtedly, the most serious crises of this era were the Jallaz (or Zallaj) Riot of November 1911 and the Trolley Boycott in the early months of 1912. The Jallaz affair demonstrated the total insensitivity of the colonial authorities in Tunis, especially that of the Résident-Général, Gabriel Alapetite. Parts of a large Muslim cemetery in the center of Tunis were to be turned over to various foreign claimants as land for commercial uses or on which to build houses. These foreigners were mostly Greeks and Italians. Under the beys of Tunis before 1881, this cemetery had traditionally been administered by the Ministry of Awqaf. Although the French resident and his council finally desisted, the Muslim population did not hear in time of their decision. A riot followed, in which two French people and an Italian were killed. Later, the mob manifested its hostility toward Italians, and as a result the Italian community in Tunis became very nervous, occasionally shooting from its houses at Muslims, even at Tunisian children.

The second episode grew out of the first, being the result of a Tunisian child being run over and killed by a trolley whose driver was an Italian. The outcome was an almost complete stoppage and boycott of the Tunisian tramways, with some other minor disturbances. Eventually, the French colonial authorities were able to exploit both events for a crackdown on the Tunisian nationalists, several of whose leaders were jailed or forced to leave the country. Even so, the nationalist reformers did win many concessions from the French. Yet nine per-

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sons (were they guilty?) were condemned to death and guillotined by these same authorities.

All of these events have been investigated in much detail by Ayadi; he is able to show what went wrong on both sides, the pressures on Tunis arising from the Italian takeover (1911) in Libya, which was going on at this time, the shabby motives of the colon group, and so on. There is an extensive bibliography in Arabic, English, and French, an index, and many contemporary photographs. It is an excellent and complete study, one of the best in this series.

B. G. MARTIN
Indiana University

JOHANNES FABIAN. *Language and Colonial Power: The Appropriation of Swahili in the Former Belgian Congo, 1880–1938*. (African Studies Series, number 48.) New York: Cambridge University Press. 1986. Pp. viii, 206. $39.50.

This book is about the growth and development of *Kizungu*, a term that Africans used to designate "the way white people talk Swahili." The vocabularies and language manuals on Katanga Swahili put out by colonial government, mission, and business concerns codified a language spoken by no one: not the Africans, who instead spoke a wide variety of context-specific versions of Swahili, and not even the whites, who quickly discarded the rules and vocabulary of the textbooks for individualized pidgins. Nor were the manuals themselves consistent. Some presented a crude pidgin that at best allowed colonizers to shout orders to African laborers; others presented a purified and rationalized Swahili that was equally remote from African speech.

Johannes Fabian's subject is the grammars, vocabularies, and textbooks by which colonizers sought to appropriate, domesticate, and ultimately control the discourse between them and their subjects. He argues that the codifying of African languages had two goals. One was practical, to facilitate at least minimal communication between colonizers and Africans. The second was to control language itself by writing it down and re-creating it in some sort of systematic manner and thus to create the illusion of power.

To trace the history of this appropriation, Fabian looks at different genres of language manuals written by colonizers in the former Belgian Congo. He argues, sensibly enough, that the succession of genres mirrored changing needs in the establishment of colonial control. Early explorers produced polyglot vocabularies with parallel lists of words in several languages, reflecting the demands of expeditions and military campaigns. As colonialism became established in the early twentieth century, there was a shift from descriptive appropriation to "prescriptive imposition and control" as missionary educational systems and colonial administration became established.

Having established the general pattern, Fabian turns to the particular issue of Katanga Swahili. Although Swahili was not indigenous to Katanga, it had nevertheless emerged among Africans by 1910 as a common means of expression for the urban life style that was developing in the mining centers. The Belgian colonizers gave it official recognition, in part to isolate Katanga from the British-controlled territories to the south. Attempts to create language manuals in order to teach Swahili to incoming Europeans resulted in varieties of *Kizungu* ranging from the crude pidgins of the early years to complicated, "improved" forms in the 1930s.

This book raises some fascinating questions about the relationship of language to power, and it gives some interesting, if incomplete answers. The ultimate conclusion must surely be that language is exceedingly difficult to control. Whatever the writers of the language manuals thought they were doing, they failed to impose their versions of Swahili even on the white colonizers.

ROBERT HARMS
Yale University

PAUL E. LOVEJOY. *Salt of the Desert Sun: A History of Salt Production and Trade in the Central Sudan*. (African Studies Series, number 46.) New York: Cambridge University Press. 1986. Pp. xvi, 351. $42.50.

More perhaps than in the modern history of any other continent, what is known about Africa stands in stark relief against a still uncertain background. That this book contains repeated references to what is not yet known and careful distinctions between established fact and even the most logical but still unproven hypothesis is a tribute to Paul E. Lovejoy's method. Years of painstaking archival and field research have resulted, however, in surely the most comprehensive monograph on the trade of the Central Sudan.

The region surveyed is, of course, the central and western geographical Sudan, not the modern republic of Sudan. Chapter 1 usefully examines types of salt and their uses: culinary, medicinal, industrial, and so forth. Chapters follow on the chemistry and geology of salt, the technology of production, and the volume of production. Chapter 6 concerns the mobilization of labor for salt production, and chapter 7 deals with proprietorship. Marketing networks are described in chapter 8, and chapter 9 is a useful summary of the political history of the region and the relationship of this to salt production and trade, the social organization of which is explored in chap-

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ter 10. General conclusions are considered in chapter 11.

Lovejoy's wealth of sources and conservative approach to them add conviction to his main conclusions: that a lack of technological advance profoundly influenced the Sudanic salt industry, that its expansion was limited by reliance on animal transport, that production and trade were confined to regional markets largely uninfluenced by outside forces before 1900, and that the salt industry was inextricably involved in the larger social and economic context of the region.

This is an exemplary study. The bibliography includes copious archival sources from Niger, Senegal, Nigeria, Ghana, and Europe, interviews with more than one hundred informants, and a full listing of unpublished and published primary sources and secondary works. Thirty-nine well-designed tables provide extremely valuable statistics, and several excellent figures and photographs illustrate the text. A special word of praise is due the four maps prepared by the Cartographic Service of York University's Department of Geography, which are models of clarity and almost flawless. The study is indexed and attractively bound.

This satisfying and admirable book sets a standard that will be difficult to match without an unusual degree of industry in research and judiciousness in writing.

M. W. DALY
Arkansas State University

HOLGER BERNT HANSEN. *Mission, Church, and State in a Colonial Setting: Uganda, 1890–1925*. New York: St. Martin's. 1984. Pp. xix, 649. $39.95.

In 1875 Henry M. Stanley entered the East African kingdom of Buganda marching behind an American flag and carrying a Bible. The Africans were unimpressed by the stars and stripes, having already designed a flag for themselves, but they took to the Bible with alarming enthusiasm. By the time Holger Bernt Hansen's admirable study opens in 1890, religious-political parties had been formed, a rival Muslim faction had been crushed (until the revival of Islam eighty years later under Idi Amin), and Christian converts had attained enough power and influence to overthrow the former system of government. They did so in favor of a Christian state in which baptism was a prerequisite for holding political office. This was followed by a brief but shocking civil war in which Roman Catholics, aided by French missionaries, fought Protestants, advised by the British Church Missionary Society (CMS). Victory for the Protestants was secured by the timely intervention of a British imperial agent firing the prototype machine gun. ("Whatever happens we have got/the Maxim Gun and they have not.") In this case they who had it not were the French missionaries and their Roman Catholic followers. So Buganda entered the colonial era dominated by a Protestant political order, advised by CMS missionaries, and allied to British secular authority.

Hansen has thus chosen a case study of exceptional interest and appropriateness for his closely focused analysis of church-state relations. His is an investigation of institutional relationships, almost entirely confined to the often divergent interests of CMS missionaries, who sought to create an established religion linked to political authority and perpetuated through education in mission schools. On the other side, the colonial government sought, with increasing success after the turn of the century, to limit the power of both the mission and the Christian chiefs. In the period before the Agreement of 1900, however, colonial agents were few in number and weak in firepower. They needed the support of the missionaries and Protestant chiefs with their well-organized and well-armed followers. This dependence was manifested in the hard bargaining over the agreement itself, from which the colonial authorities emerged with neither the land nor the direct administrative control they desired. Thereafter, as Hansen demonstrates in the major part of his work, the CMS mission (and its African allies) lost ground.

In a painstaking, detailed, and utterly convincing manner, Hansen isolates and analyzes issues of land ownership and exploitation, African labor obligations, the recognition of the mission-sponsored Native Anglican Church (NAC), and education—all areas of contention between church and state. The colonial state, in its efforts to recover from the losses of the Uganda Agreement, denied the mission the rights of landlord plantation management, with its corollary need for unpaid labor on mission estates (a practice that created many problems in neighboring Kenya). It also refused to establish the NAC but granted a significant, if temporary, monopoly of education to the Christian missions (Roman Catholic as well as Protestant). And it recognized the legality of only Christian, monogamous marriage. As time passed, the missionaries found themselves less and less at odds with the growing power of secular colonial rule and more and more in basic agreement.

When the author draws the curtain on his study, somewhat arbitrarily in 1925, the position of the Protestant establishment was no longer as secure as it had been at the turn of the century, but its influence—although considerably whittled down—was yet dominant in Buganda politics and would have a profound effect on future quasi-religious political struggles in independent Uganda.

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This is an impressively thorough study within its self-imposed limits. One might have wished for the inclusion of more African actors—chiefs and church elders—in the book, but Hansen's primary goal, which he accomplishes admirably, is to connect the sometimes arid church-state theoretical discussions to a solidly documented case study. To achieve that documentation he spent more than a decade tracking down archival and private records in Britain and East Africa. As a worthy sequel to John Taylor's pioneering study, *The Growth of the Church in Buganda* (1958), Hansen's book can safely be judged a definitive treatment of its subject. Following his lead, perhaps others will similarly treat the experience of the Muslims and Roman Catholics. Finally, the quality of the production of the book itself is outstanding—well edited and illustrated and printed on good paper, it reminds one of the standards of an earlier era.

JOHN A. ROWE
Northwestern University

DAVID MAUGHAN-BROWN. *Land, Freedom, and Fiction: History and Ideology in Kenya.* London: Zed Books; distributed by Biblio Distribution Center, Totowa, N.J. 1985. Pp. x, 284.

This is a work first of literary criticism and then of history. David Maughan-Brown examines the ties between fiction and ideology in novels about "Mau Mau" and bases his analysis on "the (materialist) recognition that . . . The literary work is just that, a work." He quotes Terry Eagleton, "Literature, like any other social practice, employs determinate means of production to transform a determinate 'raw material' into a specific product." To Maughan-Brown then, "the essential function of literary criticism is to establish the conditions of a work's production and the ways in which the work is determined by those conditions" (p. 11).

After a gelatinous Althusserian introduction (most of which Maughan-Brown thoughtfully suggests a reader uninterested in ideology or literary theory can skip), the book gathers momentum. Maughan-Brown discusses the origins and evolution of "Mau Mau" and the various "ideologies" that surround it. He then looks at the way they affect "Mau Mau" fiction.

The section entitled "'Mau Mau' as a Historical Phenomenon" (chap. 2) should interest historians most. Maughan-Brown's treatment of the issue is lucid and energetic but, alas, dated. The years since the completion of his doctoral thesis and its revision for publication have seen a renewal of scholarly interest in "Mau Mau." The result has been a series of reassessments—notably those of David Throup and of John Lonsdale—that go beyond the sources Maughan-Brown uses in his summary of the various descriptions of "Mau Mau." To cite just one example: Lonsdale has noted that, contrary to the conventional theory that the Emergency of October 1952 ignited combustible dissident groups among the Kikuyu and fused them into "Mau Mau," recent research indicates that the Emergency caused an already existing movement to cohere around a central supply center in Nairobi. In addition, it now seems that the origins of "Mau Mau" go back to at least three earlier dates: 1945, and the squatter rejection of new labor contracts; 1946, with the appearance of a new oath of loyalty among the Kikuyu; and the emergence of militant trade unionists in 1947. On these new findings, Maughan-Brown is silent.

In sum, the literary arguments in this book will undoubtedly stimulate some and annoy others, as thoughtful criticism should. But the book's historical treatment of "Mau Mau" is more limited than it surely would have been had it been published earlier—or revised later.

JOHN SPENCER
Middlebury College

MAURICE BLOCH. *From Blessing to Violence: History and Ideology in the Circumcision Ritual of the Merina of Madagascar.* (Cambridge Studies in Social Anthropology, number 61.) New York: Cambridge University Press. 1986. Pp. x, 214. Cloth $44.50, paper $13.95.

Can an essentially ahistorical purpose (concern with the *principles* of ideological transformation) and even a subject not likely to attract historians (circumcision) be treated historically by an anthropologist? Is there a basic difference in the *mentalités* of historians and anthropologists, and, if so, can disciplinary crossovers meet with reasonable success? Maurice Bloch's volume on the circumcision ritual among the Merina of central Madagascar begs the questions.

The author does not care for "particularity," described as "pointless." This is meant to explain why references are kept to a minimum and why the author is highly selective in the use of his sources, which are secondary for the most part. Bloch is also against the antihistorical Functionalists and their anthropological opponents who used "pseudo-history" to adhere to "pure theory" anyway. It is only when "real history" comes into play that we can understand how a "process of determination" ultimately produces something as complex as circumcision. Circumcision is not only a "symbolic system" but one "created in history." Thus, a study of the determining process becomes an assessment in history of the "principles of transformation." Bloch perceives "real history" as a body of written docu-

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ments about the past and is unwilling to discuss anything beyond the last two centuries, for which written documentation is extant.

Although there is an introductory chapter about the general history of the Merina, the reader will find circumcision in history only in the two concluding chapters (7–8). Such a structure prevents Bloch from using the royal and commoner circumcision ritual as a heuristic device with which to throw more light on the Merina past, an objective that is not his anyway. One of his conclusions is that, paradoxically, the history of "circumcision ceremony" over some seventeen decades (1800–1970) reveals "at once an image of stability and an image of change" (p. 157). Because the adding of a historical dimension to any study must involve attention to what is stable and what is changing in societies, Bloch's "paradox" shows that he neither thinks like a historian nor cultivates a particularly subtle understanding of our craft.

Since the early 1800s the Merina have been pivotal in the history of Madagascar—a success story matched by a vast literature that has not reached its end. Circumcision was brought into Imerina from somewhere else, it was adopted by the founder of Merina royalty and high nobility, and it intertwined with religion and became prevalent and socially important. As one would expect, royal circumcision was ceremonially the most prominent, binding monarchs and subjects, until it was abolished by a mid-nineenth-century king. From the late 1860s when Christianity became the religion of Merina state until the French annexation of Madagascar in 1896, Christian rulers and elite altered the customs of the Merina as circumcision steadily lost out. It lost, in the process, the capacity to bless, surviving only in the remote pockets of the countryside.

All of this is well known in Madagascar and by outsiders who have studied the Malagasy. Bloch adds the claim that there has been a resurgence of circumcision in Imerina since the early 1970s. In turn, he holds this to be an expression of popular and, at times, violent opposition to the rule of the Westernized elite. Neo-Marxist criticism of ruling elites in Africa is hardly a novel enterprise, and neo-Marxist *philosophisants* have been active in writing fashionable studies of "ideologies" in Africa and Madagascar. Bloch may well be right about a partial return to the "old religion" in Imerina since the early 1970s, but the claim needs to be verified. If true, however, there is at least one alternative explanation to consider. History shows that elites in Imerina have been the prime movers of change within a most conservative society. Since the days of independence from France there has been perhaps no greater national goal than to unify the various ethnicities into the Malagasy people. The Merina elite has been in the forefront of this endeavor, and it is more likely that some return to the "ancestral ways" could not avoid a ritual that once bound the state and the people in Imerina before the colonial rule.

RAYMOND K. KENT
University of California, Berkeley

ASIA AND THE EAST

K. C. CHANG, editor. *Studies of Shang Archaeology: Selected Papers from the International Conference on Shang Civilization.* New Haven: Yale University Press. 1986. Pp. xiv, 325.

This volume is composed of eleven papers chosen from those presented by forty-eight scholars at the International Conference on Shang Civilization held in Honolulu in 1982. Seven of the papers were previously published in Chinese; now translated, they are printed here along with four new papers. The heart of the book is chapters 3 through 9, which study various aspects of the magnificent materials found in the tomb of Fu Hao, a consort of a Shang king. Those chapters are first preceded by a foreword in which editor K. C. Chang describes the thematic interrelationships between all eleven chapters and then one chapter each on the earliest known Shang site and a middle Shang capital. The final two chapters are a study of the influences of Shang and Northern Zone bronzes on each other and a discussion of the typology and evolution of a bronze musical instrument in the Shang and Zhou periods. The book ends with a wide-ranging, seventeen-page bibliography and an index.

Fu Hao's tomb is almost stereotypical of the archaeological agony and ecstasy that accompany the riches of an undisturbed royal grave. The ecstasy is in the 440 bronze and 590 jade pieces and many other items that can tell so much about the lives and values of the Shang. The agony is in determining when she was buried, because a clearer understanding of many areas of the Shang revolves around this final question. The third chapter, a general study of the Shang royal cemetary system, is less directly concerned with her tomb, but it serves nicely, in a Marxian way, to set the stage for a discussion in chapter 4 of its chronological position. There the writer mainly focuses on whether or not the tomb is from the initial period of the Shang royal capital at Yin. The next four chapters investigate inscriptional evidence from within the tomb and elsewhere in consideration of the crucial question of whether Fu Hao was a personal name that referred to one specific individual or an official title used repeatedly. Chapter 5 studies the 190 inscribed bronzes from

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the tomb to determine if the various forms of names found on them could be different ways of referring to the same person. Chapter 6 pursues the topic of Shang names through the oracle bone inscriptions and pertinent literary materials, and chapter 7 focuses on a thorough examination of what we can learn about Fu Hao from the oracle inscriptions. The next chapter uses the tomb materials as part of a much broader attempt to establish a new approach that uses graphemic studies of Shang clan-sign inscriptions to determine interrelationships between bronzes and tomb occupants. Using the hundreds of jade pieces from Fu Hao's tomb to clarify nomenclature and classical literary explanations, the author of chapter 9 aims to establish new paths for the use of archaeology in studying jade.

This book should appeal to a wide audience because it approaches its topic from many vantage points and on many levels. Shang scholars will find it important because "written by a group of specialists at the forefront of new researches, it serves to define the field in essential and systematic ways" (p. ix). Nonspecialists will value the book for the thorough introductions, which then lead to in-depth studies in most chapters. As a textbook, it will be useful to archaeology students for its leitmotiv on the use of pottery, bronzes, and other materials as dating tools and for its variety of ideological perspectives. I hope students of women's issues will discover this work; it is a treasure trove of information about the status of women in ancient China. There is even a nice section on how to build a city wall.

The longest chapter, on categorizing calligraphic differences in Shang clan inscriptions, offers a promising new approach, but it is the most difficult section of the book because of its complex, and here poorly explained, classification system. This approach may ultimately prove useful, but objective criteria will be needed to define exactly what constitutes significant differences. The assumption that oracle inscriptions were first brush written and then inscribed is debatable (p. 163). There are a few typographical errors and some minor glitches, but the editor has done a fine job and produced an excellent book.

STANLEY MICKEL
Wittenberg University

ROBERT Y. ENG. *Economic Imperialism in China: Silk Production and Exports, 1861–1932*. (China Research Monograph, number 31.) Berkeley: Institute of East Asian Studies. 1986. Pp. x, 243. $15.00.

An important chapter has been added to the debate concerning the influence of imperialism on China's economic development in the nineteenth and twentieth centuries. Reviewing the copious literature on this subject, Robert Y. Eng identifies the principal parties to the debate: the revisionists, who stress the positive effects of foreign economic presence on development and the inhibiting influence of Chinese institutions, and the neo-Marxists, who see a host of foreign-induced ills such as exploitation of cheap labor and raw materials, lopsided dependence on foreign markets, and destruction of cottage industries. He selects the silk industry as a test case with which to gauge the economic impact of imperialism because of its importance as China's foremost export industry during the period, its relation to the rural and the industrializing sectors, and the parallel experience of the Chinese industry with the Japanese. Wisely, he eschews the approach that weighs the relative importance of foreign and domestic forces operating in China's economy in favor of one that seeks to comprehend the relationship and interaction between the two.

Inevitably, Eng confronts Lilian Li's study *China's Silk Trade: Traditional Industry in the Modern World, 1842–1937* (1981). Li saw China's institutional arrangements, commercial practices, and the lack of governmental leadership as decisive factors weakening the silk trade; she stressed the positive effects of foreign influence. Eng, shunning the so-called praise and blame approach of the revisionists (among whom he counts Li) and the neo-Marxists, concludes that foreign and domestic forces interacted to confer benefits and inflict injury in a complex pattern that defies easy categorization. He marshals voluminous evidence underscoring the pervasive influence of the foreign powers on the silk industry: the exercise of extraterritorial privilege to secure raw silk below free-market prices, the draining of the economy by foreign banks and shipping companies, and the transformation of whole districts in Jiangnan and Guangdong into export-dependent economies. Although he also demonstrates that the growth of the modern silk industry in response to the export market did indeed benefit the domestic economy, such benefits were eroded by the weakening of the traditional silk-weaving sector and nullified by the collapse of the world silk market in the 1930s. I was prepared for a stronger conclusion stressing the inhibiting pressure of imperialism on economic development.

By comparatively studying the two sericultural areas of Jiangnan and Guangdong, Eng has provided a valuable perspective on the industry. He has shown, for example, that circumstances in Guangdong permitted the establishment of Chinese-owned, Western-style steam filatures. Economic prosperity among the peasants in silk-producing districts in Guangdong seems to have defeated movements for rural revolution. His comparative analysis of the Chinese and Japanese industries shows further that the more aggressive ap-

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proach of the imperialist powers in China was an important factor undermining China's competitiveness in the world market. Although Eng has challenged many of Li's conclusions, the last word in this debate has not been spoken.

THOMAS L. KENNEDY
Washington State University

JOHN K. FAIRBANK and ALBERT FEUERWERKER, editors. *The Cambridge History of China*. Volume 13, part 2, *Republican China 1912–1949*. New York: Cambridge University Press. 1986. Pp. xix, 1092. $99.50.

This volume is both a continuation of the history of the republican interregnum that was begun in its predecessor (volume 12 in this series, *1912–1949*, part 1) and, in part, a summation of the history covered in the four volumes of the *Cambridge History of China* that deal with the century and one-half from 1800 to 1949. It is intended to round out the specific history of China's transition from traditional Confucian to revolutionary-socialist modes of social and political authoritarianism and to serve as a commentary on the broader historical issues of China's hesitant and painful confrontation with "modernity," defined in terms of national survival in a world changed beyond the furthest reach of inherited conventional wisdom.

In their memorable seventy-page introductory essay, "Perspectives on Modern China's History," written in happy collaboration with Mary B. Rankin, John K. Fairbank and Albert Feuerwerker characterize this larger history as "a revolutionary movement of society out from under the old state control, the emergence of new social classes, the redefinition of political relationships, and the creation of a new state structure and ideology . . . an uneven, spasmodic, and often bloody process" (pp. 49–50).

Several of the contributions to this volume more or less directly substantiate this generalization. The preceding volume on the republican period dealt sparingly with the social and popular history of these years and focused instead on "foreignism": the impact of "the West" on the economic and intellectual transformation of China's elite culture. The volume under review redresses the balance in useful, if tragic, ways. There is blood in plenty here—the blood shed in wars both civil and international. The contributions of Shinkichi Eto and Akira Iriye skillfully establish the context of China's international victimization during these years. More central to the theme of this volume, however, are Lloyd Eastman's two chapters on the rise and disintegration of the Nationalist government, Jerome Ch'en's chapter on the Communist movement from the debacle of 1927 to the establishment of the Yan'an polity in 1937, Lyman Van Slyke's on the intricate issues of war and revolution during the War of Resistance, and Suzanne Pepper's on the civil war.

"The vital entities of modern times are the peoples with their national states and cultures," wrote Fairbank in his introduction to volume 12 of the work. In this volume the contributions of Ramon Myers ("The Agrarian System"), Lucien Bianco ("Peasant Movements"), and Philip Kuhn ("The Development of Local Government") constitute a remarkably detailed survey of the fraying of the "traditional" fabric of social and economic culture and of the persistence of certain patterns of local elitist control over the resources, political energies, and lives of China's masses.

Against this background Leo Lee's continuation of his analysis of literary trends in the republican period (which, together with Lee's contribution to volume 12, constitutes probably the best available monographic treatment of the subject) and E-Tu Zen Sun's original descriptive analysis of the growth of the academic community are only apparently tangential: many of the possibilities of these decades, whether real or merely imagined, and their increasingly radical temper can only be appreciated in the context of literary and intellectual self-expression and, ultimately, embitterment and frustration.

As time goes forward, it becomes more difficult to keep the republican period in perspective as a distinct and distinctive historical episode, not merely an interval interrupting China's "inevitable" transition from Confucianism to Communism. This volume and its earlier companion are invaluable not only by reason of their scholarly substance but also as a guard against recourse to such convenient and misleading shortcuts to our understanding of China's modern transformation. China's twentieth-century revolution is unique, the editors suggest, partly because of the size of the population that it has encompassed and partly because of "the greater comprehensiveness of the changes it has confronted" (p. 2). These volumes are eloquent testimony to the magnitude, complexity, and costly difficulty of the challenge that absorbed the creative and imaginative energies of two or three generations; they are testimony, too, to substantial accomplishments as well as to the overwhelming failures that eventually moved China's history into a new and different phase.

Quite appropriately, the final note is one of uncertainty as to what is to follow. In an absorbing essay Stuart Schram discusses the evolution of Mao Tse-tung's thought to 1949: the shape of China's revolutionary future as Mao envisioned it, a peculiar blend of revolutionary experience and Marxist theory. And the final question, as Schram phrases it, is in the late months of 1987 still, or again, unresolved and troublous in respect to the logical compatibility

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of its terms and its political and social implications: "Toward a People's Democratic Modernizing Autocracy?"

JEROME B. GRIEDER
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EIICHI KIYOOKA, editor. *Fukuzawa Yukichi on Education: Selected Works*. Foreword by KAZUYOSHI NAKAYAMA. Tokyo: University of Tokyo Press; distributed by Columbia University Press, New York. 1985. Pp. xv, 280. $24.50.

No Japanese intellectual has received greater attention in the English language than Fukuzawa Yukichi (1835–1901), Meiji Japan's preeminent advocate of utilitarian liberalism and the founder of Keiō University and the daily paper *Jiji shinpō*. Fukuzawa's name appears prominently in every textbook history of modern Japan, and at least two historians, Carmen Blacker and Albert Craig, have devoted considerable energy to the analysis of his collected works. In addition we now have English translations of several of Fukuzawa's books, including his *Autobiography*, *An Encouragement of Learning*, *An Outline of the Theory of Civilization*, and a collection of his speeches. With all of this one wonders whether we need yet another translated volume of Fukuzawa's essays. The question becomes all the more pressing when considering the paucity of translated writings for other modern Japanese intellectuals, who arguably deserve as much, or even more, attention than Fukuzawa.

Such questions seem far removed from Kiyooka Eiichi and Nakayama Kazuyoshi, the editor and prologue writer for the volume under review, who approach their subject with an uncritical, almost obsequious, reverence. In his introduction Nakayama lauds Fukuzawa as an "outstanding evaluator and interpreter of Western civilization," a man who, once "liberated from the shackles of feudalism," worked tirelessly in pursuit of "dreams of freedom and independence," both for himself and his nation (pp. vii–viii). "Fukuzawa did not specialize in any one field," Nakayama proclaims, "his interest covered the whole range of civilization, and as his life work he chose to introduce the entire civilization of the modern age to the Japanese people, and that in great haste" (p. xv). Whether any human being could accomplish such a feat is dubious, but what is especially troubling about this introduction is its complete neglect of historiography. Fukuzawa's significance derives in part from the appraisals, both positive and negative, which he has received over the past half-century from such ideologically diverse historians as Hani Gorō, Tōyama Shigeki, Maruyama Masao, Yasukawa Junnosuke, Irokawa Daikichi, Hirota Masaki, Craig, Blacker, and Earl Kinmonth. To dismiss the opinions of these and other historians and to reduce Fukuzawa to an insipid archetype of the benevolent wise man add little to our understanding of the complexities of early Meiji Japan.

An uninspired introduction, however, should not discourage potential readers from examining Fukuzawa's own words of advice about education. True, several of this volume's excerpts, notably three essays from *An Encouragement of Learning* and one from *An Outline of the Theory of Civilization*, have been translated before, but much of the remainder, including interesting tidbits from Fukuzawa's letters, speeches, school regulations, and moral discourses, appears in English for the first time and in a literary style that is generally smooth and convincing. With his repeated calls for a practical learning that would nourish "the principle of independence and self-respect," Fukuzawa's familiar voice reverberates clearly throughout this volume for the benefit of all students of nineteenth-century thought.

DONALD RODEN
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SHUNSUKE TSURUMI. *An Intellectual History of Wartime Japan, 1931–1945*. (Japanese Studies.) New York: KPI. 1986. Pp. viii, 136. $39.95.

Shunsuke Tsurumi is one of the leading philosophers and social critics in Japan today. In this slim volume, based on a series of lectures delivered at McGill University in the fall of 1979, Tsurumi presents us with a vivid account of the intellectual atmosphere in Japan between the Manchurian Incident and Japan's defeat in World War II. He does this by focusing on *tenkō*, whereby the majority of Japanese leftists and liberals converted to nationalism in response to social pressures and governmental intimidation in the 1930s. According to Tsurumi, *tenkō* is not a peculiarly Japanese phenomenon. A comparative study ("*tenkō*-logy") would reveal similar phenomena in other countries, such as Stalinism in Europe and McCarthyism in the United States.

Although the author does not mention himself, he and his family could serve as an example of what happened to Japanese intellectuals during and after the war. His father, Yusuke Tsurumi, was a liberal writer and roving ambassador, who admired the United States but ultimately came to defend the position of the Japanese government. The author, a 1942 graduate of Harvard University's Department of Philosophy, was repatriated after the outbreak of the war and served as a civilian employee of the Japanese navy. (His sister, Kazuko Tsurumi, a Princeton Ph.D. and professor of sociology, is well known in the West for her writings on Japanese society during and after the war.)

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The book's title is somewhat misleading, in that the book is not really an intellectual history of wartime Japan. It is rather a collection of interesting observations, mostly on the intellectual background to the period that the author, like other Japanese historians, labels "Japan's Fifteen Years' War" (although by Western reckoning the period lasted somewhat less than fourteen years). Among the other topics discussed by the author are the plight of the Koreans, the resistance to the war by some little-known religious groups, the problems of Okinawa, the student riots of 1960, the antinuclear campaign, and the antipollution movement. These essays were originally published in Japanese under the title *Sejiki Nihon no seishin-shi* (Ideological History of Wartime Japan [1982]), and the Japanese publication was awarded the Osaragi Jiro prize. Written by an expert insider and addressed to the general reader, the book is a valuable addition to the literature on Japan before, during, and after World War II.

BEN-AMI SHILLONY
Hebrew University of Jerusalem

TERUO GOTODA. *The Local Politics of Kyoto*. (Japan Research Monograph, number 7.) Berkeley: Institute of East Asian Studies, University of California, Berkeley. 1985. Pp. xii, 161.

This well-made paperback conveys research findings in recent Japanese local history of a sort unlikely to appear in journals or hardcover books. Teruo Gotoda makes this report on post-World War II Kyoto politics long enough to be detailed yet trenchant so that a reader can easily grasp the main issues.

Gotoda focuses his study on the seven-term gubernatorial career of Ninagawa Torazō, in Kyoto Prefecture, lasting from 1950 to 1978. Gotoda examines the components of this success story in chapters on linkage politics, political organizations, personalities, and the role of intellectuals. He concludes that democracy "is still inchoate in Japan" (p. 21). Leaders rise up from bureaucratic moorings and do nothing to rock the boat, preferring to stay in power and avoid tough issues. "Thus, the Japanese model is predominantly one of bureaucratic, rather than democratic rule in substance" (p. 21). And Kyoto is no exception.

Arguing that Kyoto has approximated a "mass society" since Muromachi times (the fourteenth century!) in its "cultural uniformity" (p. 5), Gotoda finds Max Weber's model of public administration and bureaucratic control more relevant than a Marxist approach to "the ruling class." Along the way he suggests that local and regional differences vitiate the stereotype of Japan as a homogeneous society and that its touted social harmony is hard to find. "The Japanese are just as interested in individual advantage as Americans" (p. 129). The ancient capital of Kyoto shares general Japanese themes, suffering typically at the hands of Tokyo-oriented national officials yet continuing to support venal politicians as do other Japanese communities.

We have here what amounts to an anecdotal history of politics in postwar Kyoto. Gotoda repeatedly employs the casual anecdote in place of rigorous analysis to make a point. Kyoto people differ from those of Osaka and Kobe, he says, in their attitude toward officials, and he supports his view with a shaggy-dog story about the way a common taxpayer will approach a tax collector in the three Kansai cities. Japanese leadership consists of unintelligent, grasping politicians, he says, and he depicts leaders according to the Edo-period model of "imbecile lords" and "portable shrines" (*mikoshi*) who are prominent but irresponsible figureheads.

Longtime governor Ninagawa, however, was no *mikoshi*; he brought real talent to Kyoto and national politics. But, if this is so, Gotoda's contempt for Japanese leaders would appear to be misplaced. Anyway, it is unedifying to run into an unremitting caricature of political leaders despite their accomplishments and the prosperity of the people they govern. Democracy is imperfect everywhere, but why excoriate Japan's postwar politicians as if they had never achieved anything? The anecdotal approach detracts from the book's persuasive power and mars an otherwise informed effort to convey the lessons of postwar Japanese politics through a study of the Kyoto experience.

GEORGE M. WILSON
Indiana University

L. DE JONG. *Het Koninkrijk der Nederlanden in de Tweede Wereldoorlog* [The Kingdom of the Netherlands in World War II]. Volume 11b, parts 1 and 2, *Nederlands-Indië II* [The Netherlands Indies, II]. Leiden: Martinus Nijhoff. 1985. Pp. viii, 507; vi, 509–1116.

Eighteen years have already passed since the first volume of L. De Jong's *Koninkrijk der Nederlanden in de Tweede Wereldoorlog* appeared. This great work is now nearing completion. The last three volumes discuss the history of the Netherlands Indies during World War II and the return of the Dutch colonial government in late 1945. The numbering of these last three volumes might cause some confusion. Volumes 11a and b, each of which consists of two parts, are also designated as volumes I and II. Volume 11c, which was published in 1986, is also volume III. Thus, we have a total of three volumes

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or five parts with a total of some twenty-nine hundred pages.

As in the previous volumes, De Jong has masterly brought together and interpreted his material and presented a well-organized and well-written account of this fascinating but also depressing aspect of contemporary history. De Jong was not faced with a paucity of materials when he wrote his history of the German occupation of the Netherlands; he had a far more difficult task reconstructing the history of the Japanese occupation of Indonesia. Unlike the Germans, the Japanese left behind little documentary material. The Indonesians and the Netherlanders also did not leave many records. Yet, by using a limited number of Dutch and other sources and a few important secondary works, De Jong is able to give us a fairly good description of the Japanese occupation. The story that unfolds is rather depressing, and, after having read De Jong's absorbing account, I found it difficult to shake off a feeling of revulsion over Japanese brutality and disappointment over the lack of native resistance against the new masters. Although De Jong does not give a detailed picture of what life was like for an average Indonesian during the Japanese occupation, he provides enough information to enable us to conclude that conditions were extremely harsh and brutal. Yet, unlike other people of Southeast Asia who suffered under Japanese rule, Indonesians offered little organized resistance. Why was this so? Was it because of their long history of submission to colonial rule? Or was it because of their geographical isolation and the lack of Allied encouragement and assistance? Understandably, De Jong concentrates much of his attention on the fate of the European population. Forty-two thousand two hundred thirty-three European prisoners of war were in Indonesia, Japan, or elsewhere and often were forced to perform labor under the most brutal circumstances. As a result some eighty-two hundred men died, many of whom perished in Burma where they had to build that infamous railroad. Civilian European internees fared a little better. Of the ninety-six thousand to one hundred thousand who were interned, 13.6 percent died as a result of callous, brutal, and sadistic treatment and the lack of medical care and food. The history of the POW and civilian internment camps, so graphically described by De Jong, still fills many with anger and anguish. Thus, it came as no surprise that in 1986 the Netherlands government decided not to approve a royal visit to Japan for fear that such a gesture would open old wounds and stir old animosities.

The last chapter of De Jong's work discusses Sukarno's collaboration and the events preceding the proclamation of the Indonesian Republic. Much of this story has been told before, but it is good to be reminded of Sukarno's opportunism, his firm belief in Japanese victory, his hesitancy in August 1945, and the role of Indonesian youth in forcing his hand. Thus, on their return in late 1945 Dutch officials and forces found a new and very complex situation: a partially militarized and politicized population and a new regime that enjoyed considerable popular support. Such were some of the legacies of the Japanese occupation.

GERLOF D. HOMAN
Illinois State University

LEIGH ASTBURY. *City Bushmen: The Heidelberg School and the Rural Mythology*. New York: Oxford University Press. 1985. Pp. vii, 216. $49.95.

This fascinating study brings together an analysis of artistic style, social and political process, popular culture, and national mythology in relation to one of the most distinctive periods of Australian art history.

The "Heidelberg school" originally referred to a group of artists who painted plein-air landscapes in the Heidelberg area of Melbourne in the 1880s and 1890s. However, it came to be used as a loose label for those who provided a transformative vision of Australian life, marked by large-scale, subtly lighted paintings of heroic, distinctively Australian figures engaged in a complex, ambiguous relationship with nature, at once conquerors and conquered. The work of this school has long been taken to mark a fundamental turning point in Australian self-depiction, showing a rebellion against the conservative academic traditions of nineteenth-century European art and a new apprehension of the Australian landscape itself, through which an emergent national identity could be established.

In this complex, dense, and rewarding study Leigh Astbury argues for an alternative view. He suggests that the Heidelberg artists reflected the tastes and values of urban society, rather than a rural reality; thus, the pastoral life depicted was mythical and was invoked as a marker of genuine Australian identity at the very moment when the authentic rural traditions were passing away. He argues also that there was no "sudden breakthrough" but a cumulative development of visual representations based on earlier colonial traditions in art. With carefully chosen and very informative illustrations, he demonstrates the importance of both black-and-white illustration and photography in the emergence of the Heidelberg "vision" and the influence of European art movements, in particular of the French and English realist painters of idealized rural life.

A study of the Heidelberg school is not merely an exercise in antiquarianism. Nationalistic visions of the late nineteenth century have reemerged in Australian popular culture and mythology, not to men-

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tion art markets, in the past decade, and the works of Arthur Streeton, Tom Roberts, and, particularly, Frederick McCubbin have reoccupied a notable place in popular representation, with prints of their works selling in the thousands and the originals fetching extraordinary prices.

Each chapter of the book reveals a fascinating dimension linking the art of the period with events of the time. The relationship between the popular media and the development of the figure is particularly revealing, for example in the way American frontier imagery, particularly that of the cowboy, was absorbed and translated into the image of the Australian bushman because of the popularity of magazines such as *Century* and *Harper's*. This male figure on horseback, silhouetted against a vast sky, surrounded by cattle, epitomized at once resourcefulness, independence, and the same kind of freedom that another Australian figure, the swagman, also signified. The bushman "on the wallaby track" became a prime representative of the democratic, egalitarian life style, free of any master, which somehow came to represent the genuine Australian ethos. These were men, rather like "tramps" in the U.S. positive connotation, who traveled freely from place to place, exchanging their labor only for the needs of the day and then moving on again through the empty outback spaces. As Astbury remarks, the realities of pastoral life at the time, including the repressive Master and Servant Act, which regulated the conditions of rural labor, did not appear in this depiction. Neither did the enormous struggles between capital and labor, including the violent dramas of the shearers' strikes, form any part of the background of the popular large-scale paintings of men shearing sheep on which all Australian children were brought up.

Particularly striking is the fact that, at the very time when this roving rural life was being most glorified, the ideal of home ownership in the cities was at its height. In the 1880s Melbourne enjoyed the highest rate of home ownership of any comparable city in the world—by 1891, 41 percent of Melbourne householders owned or were buying their own home (pp. 80–81). Thus, the idealized "freedom" of the bushman was a freedom from the burdensome commitments of the rising urban petite bourgeoisie; possessing nothing but bedroll and billy-can, he could remind them at once of the economic independence they would never have and of the level of security in material possessions that was the reward for their loss of freedom.

The image of the lost child is another with deep resonances in Australian culture, posing as it does the immensity and dangers of the unconquered wilderness against the gentle innocence of the child seduced by its wildflowers into peril. The basis of this in real historical events of lost children is provided in Astbury's account as is the role of the aboriginal "black-tracker" in locating and rescuing the children. In many respects the Australian national identity itself partakes of the image of the lost child, parted from the lineage of the mother country, innocently proud of its independence yet unaware of the dangers such independence entails.

Mention of aborigines raises two omissions in the book: first, there is no systematic discussion of the absence of aborigines and other minorities, such as Chinese, from depictions of the period (during which Australian racism was at its height), and, second, the position of women is touched on cursorily but not developed. The visual dominance of images of women suffering, lonely, lost and pathetic, in the form of the young widow, the exhausted wife of the pioneer, the frightened mother of lost children, suggests a specific locus for the "feminine" within the development of the national ethos.

The oddest omission in the book, however, is its ending or rather the lack of it. I turned the page looking for a concluding chapter to tie the various themes together but found only the end notes. This abrupt, unfinished conclusion mars the work and is much to be regretted.

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Macquarie University

UNITED STATES

ARTHUR M. SCHLESINGER, JR. *The Cycles of American History*. Boston: Houghton Mifflin. 1986. Pp. xiii, 498. $22.95.

The collected essays of historians, like the retrospectives of artists, serve to remind us of their past themes and to reveal the overall direction of their work. That is particularly true of these pieces, notable for their style, wide-ranging quotations, and breadth of inquiry. Arthur M. Schlesinger, jr., no friend of either the New Left or New Right, is an advocate of progressive reform like his father, Arthur M. Schlesinger, Sr., and of the democratic system of checks and balances in the tradition of James Madison and the founding fathers.

Schlesinger's commentary on the "cycles of American history" is significant less for its calculations and predictions than for the assertion that the national polity has alternated between periods of reform and of consolidation or reaction. He sees a dichotomy too in the vision of America. John Winthrop's original view of "the corruptibility of men and the vulnerability of states"—of America as experiment—gave way to "the delusion of a sacred mission and a sanctified destiny," a mystic notion persisting

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through the presidencies of Woodrow Wilson, Lyndon Johnson, and Ronald Reagan (p. 16). This dogma Schlesinger considers particularly offensive when applied to foreign affairs in tones of superior righteousness. Although he favors individual advocacy of human rights, he feels they must find their sanction in laws and institutions, not in unilateral government action. On the other hand, he is unsympathetic toward the allegations of New Left historians that the United States was responsible for instigating the cold war; rather it grew out of misunderstandings and irreconcilable views in both Washington and Moscow. As for the "American empire" that the New Left inveighs against, at its worst it is the product of Third World leaders bamboozling Uncle Sam to gain assistance, and at best it is as much other nations seeking protection as it is the United States trying to dominate them.

Schlesinger is not sympathetic toward sweeping proposals for modification of the Constitution. He does favor abolishing the office of vice president, which tends to emasculate the holder, and shortening the interregnum between the election and inauguration of a president. His other suggestions are more conservative: to rescind the no-third-term and presidential succession amendments, alter only slightly the electoral system, and not tinker with separation of powers. He also does not long for the restoration of highly disciplined, old-fashioned political parties in which insiders anonymously wielded power. "The parties were their own grave-diggers," he notes (p. 266). Changes within them will continue; probably party names will remain while structures evolve to meet new needs. "The power of American democracy lies in its capacity for adaptation" (p. 275).

Overall, Schlesinger is optimistic about the American system thanks to its countervailing forces: "Democracy, properly construed, assumes neither total perfectibility nor total depravity . . . The democratic way by no means guarantees the triumph of virtue . . . Still, the exercise of dissent and opposition tempers the delusions of power" (p. 434).

FRANK FREIDEL
Harvard University

DAVID A. J. RICHARDS. *Toleration and the Constitution*. New York: Oxford University Press. 1986. Pp. xvii, 348. $29.95.

This is not a historical monograph. David A. J. Richards is a law professor, and his stated purpose is to develop a new approach to constitutional interpretation. History is one of the components out of which he fabricates an innovative methodology, and his acknowledgments thank noted historians John Reid, William E. Nelson, and Harold Hyman for their advice. This book contains far more philosophy than history, however. What Richards attempts to do is to "integrate a critical political theory of our constitutional law with a self-understanding of our historical constitutional traditions, and the larger moral, religious, and political ideals they reflect" (p. vii). His labors yield a book more properly catalogued under "jurisprudence" than "legal history."

Richards's thesis is that for Americans the central constitutional ideal has always been toleration. He builds a case for this proposition out of some history and much philosophy. After fashioning a contractarian political theory of equal respect for conscience, he argues that, suitably developed, his creation illuminates a common thread of argument underlying such constitutional rights as religious liberty, freedom of expression, and personal privacy. Richards also employs his theory as a standard against which to evaluate critically many decisions rendered by the U.S. Supreme Court.

For him, history is useful mainly as an instrument with which to establish the centrality of toleration in the American constitutional tradition. Yet, even when explaining the purpose and meaning of provisions of the Constitution, he relies more on political theory than on historical research. Richards is openly contemptuous of the "strict constructionist" approach to constitutional interpretation championed by legal historian Raoul Berger of the Harvard Law School—and, he might have added, by Attorney General Edwin Meese. Meese and Berger advocate poking around in the past in search of the "true intent" of those who framed such provisions as the Fourteenth Amendment and insist that these provisions may not be properly applied to accomplish anything beyond the specific purposes their framers had in mind. Richards charges that such historical strict constructionism "rests on bad philosophy" (p. 35). It is his contention that those who wrote important parts of the Constitution deliberately employed abstract language. Consequently, Richards argues, it would be unreasonable to restrict ourselves forever to the historic denotations of the words they used. Rather, constitutional interpretation should strive for consistency with the principles the Framers were trying to implement.

Richards offers a potent critique of Berger's vastly different approach to history, and his command of the literature on subjects ranging from religious history, through political theory, to human sexuality is impressive. His historical research is not. It is limited mainly to secondary works, supplemented with a few published primary sources. He has written a book that deserves the serious attention of constitutional scholars and judges. Some historians may find it interesting too, but few of them will learn

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much from this book about history that they did not already know.

MICHAL R. BELKNAP
California Western School of Law

MICHAEL KAMMEN. *A Machine That Would Go of Itself: The Constitution in American Culture*. New York: Alfred A. Knopf. 1986. Pp. xxii, 532. $29.95.

Practitioners of constitutional history long ago abandoned—or should have—attempts to treat their subject *in vacuo*. Thus, we have had substantial studies of constitutional development that relate to political, economic, or social phenomena. And now, Michael Kammen has focused his vast learning and erudition on the cultural impact of the American Constitution. The result is unique, refreshing, and highly rewarding.

Kammen offers a dazzling array of pertinent quotations, carefully assembled to reflect shifting cultural perceptions of the U.S. Constitution. His reach is remarkable. Besides the obvious manuscript sources of James Madison, Thomas Jefferson, John Marshall, Abraham Lincoln, Oliver Wendell Holmes, and William H. Taft, Kammen has exploited unusual, hitherto unused records, such as those of the architect of the Supreme Court and the Sesquicentennial Commission. He has intelligently used recent public opinion polls for some insights into general public attitudes. (This attempt to understand what "ordinary" people knew about the Constitution might have been still further enriched by an examination of widely used twentieth-century public school texts, such as the volumes by David Muzzey and Merle Curti, and supplementary materials such as *My Weekly Reader* and *Young America*.)

Kammen shrewdly distinguishes between the history of the Constitution and constitutionalism. Americans' veneration of the Constitution is for the document that orders our public life. Constitutionalism, as Kammen notes, "embodies a set of values, a range of options, and a means of resolving conflicts within a framework of consensus. It has supplied stability and continuity to a degree the framers could barely have imagined." Constitutionalism, in short, has both served and preserved the Constitution.

Samuel Adams anticipated the American love affair with binding, higher law when he told Joseph Warren in 1774 that "there is a charm in the word constitutional." We use it, as Kammen correctly notes, as "a convenient camouflage for moral compromise and political expediency." Thus, segregation has been both constitutional and unconstitutional. Barry Goldwater assaulted the Supreme Court in 1964 for its earlier decision prohibiting prayer in the public schools because it suited his constituency. But twenty years later, he forthrightly opposed a constitutional amendment that would have permitted such prayers. Somewhere in the intervening period, the senator must have decided that our political religion was the more perceptible mandate of heaven.

In 1838 Lincoln expressed the hope that the American Constitution would "become the political religion of the nation." Prescient as he was, Lincoln had only to listen to the constitutional cant that surrounded him to know that different perceptions dictated a multitude of sects, each claiming a hold on the one true faith. And the struggle and divisions continue today, as we divide over a multitude of issues, ranging from presidential power in foreign affairs to affirmative action. In his discussion of the sesquicentennial festivities of 1937, Kammen observes that "Americans took great pride in their Constitution, but still hadn't learned that it was not, and is not, a machine that would go of itself." If Attorney General Edwin Meese III and his supporters have their way, the present bicentennial celebration may not take us much further. Their demand for a return to a "jurisprudence of original intention" reflects a static view that contradicts and distorts historical reality but usefully legitimates their political agenda.

Yet the Constitution persists, in an almost mystical way, as the binding glue. A century ago James Bryce said that there were times "when things would have gone badly had it not been for the respect . . . formed for the Constitution." In other words, the Constitution has been the machine, instrument, or organism—any of Kammen's metaphors will do—for providing the articles of social peace to tame the pluralist wars and tensions that rage just beneath the surface of American life. And all that despite the often monumental ignorance and misunderstanding of the Constitution that Kammen has ably chronicled.

STANLEY I. KUTLER
University of Wisconsin

WILLIAM H. GOETZMANN and WILLIAM N. GOETZMANN. *The West of the Imagination*. New York: W. W. Norton. 1986. Pp. xviii, 458. $34.95.

William H. Goetzmann and William N. Goetzmann, father and son, have authored a rich and remarkable study that surpasses the six-part documentary that shared the same title as their book. This judgment is not a discredit to that series, which first appeared on national public television in the fall of 1986. As the Goetzmanns explain in their preface, the book "at first paralleled the television series, then became its chief research resource, and finally transcended the series in scope" (p. xviii). Readers

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who have seen the series will be doubly rewarded, but six hours of video viewing is not a prerequisite. The book stands on its own merit. Besides, the book's numerous color and black-and-white illustrations are so well done that they invite a careful, even leisurely, examination that a video presentation does not allow.

The elder Goetzmann is professor of American studies and history at the University of Texas. His *Exploration and Empire: The Role of the Explorer and the Scientist in the Winning of the American West* received the 1967 Pulitzer Prize. His son, a documentary scriptwriter and filmmaker, has been director of Denver's Museum of Western Art. Their book is part of a grand tradition of studies that have considered "the West as people imagined it" (p. xv).

Henry Nash Smith presented the West as myth and symbol in his pathbreaking volume, *Virgin Land* (1950). The Goetzmanns also are concerned with myths, but their presentation does not follow Smith's examination of predominantly nineteenth-century popular writings and culture. Instead, they examine the visual images of the West in paintings, drawings, cartoons, photographs, and film. A wide range of nineteenth- and twentieth-century artists is represented—from Charles Willson Peale's artistic explorations of science to Georgia O'Keeffe's "transcendent vision" (p. 433).

At times the Goetzmanns imply that visual images may have a greater impact on popular consciousness than the verbal images studied by Smith and others. For example, the Goetzmans claim that the wide distribution of pictures by George Catlin and Karl Bodmer, which depicted mounted Plains Indians, "began a stereotype that was . . . reinforced many times over by later painters" and obscured "the eastern woodlands Indian without the horse . . . iconographically" (p. 56). Similarly, Alfred Jacob Miller's "unforgettable images" of fur trappers "helped to create . . . the Far West's first authentic hero and to visualize in detail the very beginnings of the Anglo myth of the West" (p. 60).

This book may not appeal to the specialist who desires an extended series of essays in art criticism. Nonetheless, the Goetzmanns present detailed discussions of numerous artistic works—such as the political paintings of antebellum Missouri done by George Caleb Bingham and the "near-gallows humor" (p. 407) of the Indian portraits done by T. C. Cannon, a Kiowa-Caddo who died in 1978. Stimulating insights also abound, such as the comparison of Frederic Remington and Charles Russell with Walt Disney (p. 283).

In sum, this book has both visual and intellectual rewards for all readers interested in American culture.

CLYDE A. MILNER II
Utah State University

LLOYD C. GARDNER, editor. *Redefining the Past: Essays in Diplomatic History in Honor of William Appleman Williams*. Corvallis: Oregon State University Press. 1986. Pp. x, 258. $27.95.

However William Appleman Williams may ultimately be judged as a historian, he will surely be cited as a bold, original, and imaginative thinker with a passion for history's relevance, a devotion to teaching, and an enormous capacity for friendship. His generosity and loyalty over the years have prompted colleagues, students, and co-workers to honor him with that most treacherous of tributes, a festschrift. It contains twelve essays, a minimum of editorial remarks, a comprehensive but not complete list of his publications, plus ten curiously selected works of evaluation. Of the twelve authors, ten have been connected with the University of Wisconsin; an eleventh is a colleague at Oregon State University, where the enterprise originated and was carried out.

The first four essays focus on Williams. William G. Robbins uses letters, interviews, and personal contacts to present an informative and affectionate sketch of the man and his career, but he lacks a sure touch when dealing with some aspects of the historical profession. Bradford Perkins reprints from *Reviews in American History* for March 1984 a superb analysis of the impact of *The Tragedy of American Diplomacy* but, as in the original version, does not have space to deal with the contents. Ivan R. Dee tells of Williams's assistance in the 1960s to Quadrangle Books, the publisher of many works by self-styled revisionists. David W. Noble draws on a chapter in his *The End of American History* (1985) to place Williams in the broad sweep of American historiography.

The other eight are interpretive essays on various topics. The most valuable are by Walter LaFeber, Lloyd C. Gardner, and Thomas J. McCormick. LaFeber traces with a critical eye the Monroe Doctrine from its antecedents to the latest distortions of the Reagan era. Gardner exploits fresh sources in discussing American strategic thinking to 1954, emphasizing the dilemma posed by nuclear weapons. McCormick places the United States as a hegemonic power in its historical and global setting. Fred Harvey Harrington stresses the "Europe first" orientation of American diplomacy; despite a generous recognition of works by his own students, Williams's mentor might have written much of this essay in 1950. Carl Parrini's "Theories of Imperialism" is largely concerned with the problem of surplus capital. Edward Crapol argues that by 1840 antislavery forces had developed a set of foreign policy demands that were co-opted by John Tyler and James K. Polk. Relying on familiar materials, Patrick J. Hearden outlines Herbert Hoover's dream of capi-

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talism in one country. Margaret R. Morley's sketch of Freda Kirchwey as a cold-war critic is based on the columns of the *Nation* and a few items in the Kirchwey papers, the location of which is never given.

Festschrifts rarely satisfy everybody. This one could have been improved by including essays that reflected more directly Williams's concepts and methods. Only McCormick's meets that requirement, and even his seems more indebted to Immanuel Wallerstein and Nigel Harris than to Williams. One looks in vain for a picture of Williams's seminar at Wisconsin similar to that LaFeber drew of Harrington's at the 1985 meeting of the Organization of American Historians. Missing, too, is the self-criticism evident in Williams's preface to *The Roots of the Modern American Empire* (1960). Finally, more might have been made of Williams's collegiality and tolerance as expressed in a 1974 note to this reviewer. It ended with "whatever our differences, and they probably are not quite as great as either of us sometimes think, it is good to be in an adventure with the likes of you."

RICHARD W. LEOPOLD
Northwestern University

JIMMY M. SKAGGS. *Prime Cut: Livestock Raising and Meatpacking in the United States, 1607–1983*. College Station: Texas A&M University Press. 1986. Pp. xiii, 263. $28.50.

This is an audacious book. With buccaneer-like dash, Jimmy M. Skaggs slashes his way through more than three hundred fifty years of agricultural history. Actually, the first two hundred-plus years of the history of the livestock and meat-packing industries are dispensed with in two chapters. It takes Skaggs five chapters to cover "the rest of the story." That he does it as well as he does is an obvious testimony to Skaggs's mastery of the literature of these two industries. The thirty-page bibliography represents more than a bow to the ego: Skaggs knows what is between the covers of those volumes. He announces his thesis forthwith, specifically, that the "red-meat industry" is a microcosm of the American economy—as diffuse and ever-changing as a kaleidoscope. As Skaggs notes, those three centuries of history are packed with scandal, technological innovation, evolving consumer preferences, conflict between government and private enterprise, oligopoly, and the attendant complexities of the Industrial Revolution in America.

In relating this economic tale, Skaggs skips from peak to peak, providing his readers with a graphic chart that vividly records the valleys and the pinnacles of the red-meat industry. The ease and expertise with which Skaggs interprets these intricacies of his history is beautifully revealed in a gem-like paragraph on page 61, where the author summarizes, in three sentences, the history of the range cattle industry for generations.

The transportation revolution forced the livestock and meat-packing industries into the national economy. After the Gilded Age the answers as well as the problems of the red-meat industries were inexorably linked to the national economy. Nothing could demonstrate the shadow of that economic relationship more, as Skaggs observes, than the oligopolistic nature of the infamous "beef trust." These few sellers (the meat packers) dominated the marketplace of millions of consumers. Just as crucial, as many consumers realize yet few state, is the reverse: the domination of the producers via these same packers. Of course, these two economic marketing themes rule much of the agricultural economy.

What of the future? Skaggs is not optimistic, but it is difficult to dissent from his realism, although some may define it as pessimism. He foresees a future in which livestock producers, in competition with each other as well as with foreign competitors, will become increasingly marginal. The implication then is clear: following a pattern set by the rest of the agrarian economy, fewer and fewer will be producing more and more. As for the meat-packing industry, "The trend unmistakably points toward a new meat trust" (p. 218). One of the major ironies of the agricultural history of the past century is thereby developed from a negative to a positive print. What producers and consumers have protested and fought against (for more than one hundred years), albeit in a disorganized fashion, is about to come to pass. This time their voices will be drowned in the international marketplace; silence will reign supreme.

Inevitably, in a survey of this nature, any reader is going to wish for the treatment of topics untouched by the author's hands. I would have appreciated more insight into livestock financing and the problem of federalism against the agricultural background. But much must be left out, and easy shots are left to those who could accomplish more. Within his prescribed limits, Skaggs has presented a major work that will grace the reading lists of the sons and daughters of his producers and consumers.

GENE M. GRESSLEY
University of Wyoming

CALVIN MARTIN, editor. *The American Indian and the Problem of History*. New York: Oxford University Press. 1987. Pp. xiv, 232. Cloth $24.95, paper $9.95.

Calvin Martin is a young scholar obsessed with an idea. He considers American Indian cultures radi-

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cally different from white culture, and he wants to convince historians that it is impossible to write about Indian-white relations, to say nothing of Indian history itself, unless one understands and adopts the Indian world view. He began his campaign in 1981 with an essay in *Ethnohistory* entitled "The Metaphysics of Writing Indian-White History," in which he chided historians for whom the white thought world had "subtly transgressed its explanatory boundaries to pose as the sole or only valid or only serious explanation of what transpired when Indian and white met" (p. 28). He says that the "chief aim in life in virtually all North American Indian societies was to be saturated with the primordial Power of Nature which seemed to pulsate throughout all creation" (p. 29). Indians, Martin argues, are creatures of a mythic world; it and the white world appear to be "mutually irreconcilable, mutually antagonistic, mutually unintelligible" (p. 33). He wants historians to lead the way "in dismantling the white paternalistic mind-set, that in its expression has always crippled the Indian, by admitting that there is another legitimate way of interpreting our mutual past" (p. 33).

To promote this process, Martin in 1982 sent a copy of his article to a number of historians, anthropologists, and writers who have dealt with American Indians, asking them to react to his piece, "to stand back and survey the whole five-hundred-year panorama of joint Indian and white history . . . [and] to distill what they considered to be the essence of the Indian-white dialogue" (p. 5).

The present book contains the short essays of the eighteen scholars who chose to respond. It is a very diverse collection, as one might expect. Most of the authors do not follow Martin's prescription precisely; few, indeed, directly react to his article; and some reject his views. Robert F. Berkhofer, Jr., for example, renews his plea for a "new Indian history" that is not entirely white-oriented, but he questions whether it can completely escape the restraints of white perceptions of history. The Canadian scholar Cornelius J. Jaenen makes a strong plea for historical understanding of both Indians and whites in their historic times and debunks pro-Indian stereotypes accepted by ethnologists and historians today. Henry F. Dobyns argues for a truly scientific history, based first of all on demography, not for a comparing of cosmologies in a literary fashion. Mary Young talks about the Cherokees and their various responses to white pressures but insists that it is impossible to find simply one single Indian world view. Wilcomb E. Washburn retraces his fight with the radical historians, and Vine Deloria, Jr., pleads for accurate historical information in order to destroy erroneous conceptions about Indian-white relations. Some of the other essays, such as those by Christopher Vecsey, Peter Iverson, Peter Nabokov, N. Scott Momaday, Robin Ridington, and Henrietta Whiteman, are consciously autobiographical, tales of how they encountered or experienced Indian ways and how they gained an appreciation of them. Gerald Vizenor ends the collection with a fictional literary piece.

There is no thematic development in the combined essays, yet the tone is one of agreement that the Indian side as well as the white side must be considered in writing about Indian-white encounters. The authors are experienced and skilled writers, and their essays generally make excellent reading. One can pick up the book and read the chapters in random fashion, profiting from the considered reflections of men and women who are respected scholars in Indian matters.

I wish that Martin, as editor, had ended the book here, with his original article (printed as the first chapter) and the essays of the eighteen contributors. Instead, he has supplied a long introduction and an even longer epilogue. He uses these additions in part to discuss the essays in the book but chiefly to advance the ideas adumbrated in his article. He sketches an elaborate case for Indians as biological (emphasizing nature) and Europeans as anthropological (emphasizing humanity). There are some striking points (although I myself am not drawn to them), but the two long pieces unfortunately are not finished work; they are repetitious, rambling, and often a stringing together of notes taken from a variety of sources (some of them not from the writers themselves but from secondary accounts of their thought). Large sections are thus drawn from other works, among which Mircea Eliade's *Cosmos and History* (1959) and Douglas Kellogg Wood's *Men against Time: Nicolas Berdyaev, T. S. Eliot, Aldous Huxley, and C. G. Jung* (1982) figure prominently. This scissors-and-paste approach and the complicated ideas make these essays hard going.

Martin's innovative approach needs further development based on independent research on Indian cultures before it can be fully evaluated. Then his work may help us understand and appreciate the Indians with whom the whites originally came in contact. But I worry that his heavy emphasis on the differences that divide Indians and whites might actually obstruct an understanding of the relations between the two races in historic times and especially in our own age, when cooperation and joint action between Indians and whites are of such great importance.

FRANCIS PAUL PRUCHA
Marquette University

WALTER L. WILLIAMS. *The Spirit and the Flesh: Sexual Diversity in American Indian Culture*. Boston: Beacon. 1986. Pp. 344. $21.95.

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In the early eighteenth century the Jesuit missionary Pierre François Xavier de Charlevoix described with some outrage the behavior of Mississippi Valley Indians in which certain men dressed as women, engaged in women's "occupations," and interacted sexually with men: "it was pretended that this custom comes from . . . religion . . . If the custom . . . had its beginnings in the spirit, it has ended in the flesh." In this book Walter L. Williams examines the native American institution of the berdache—"a morphological male who does not fit a society's standard man's role, who has a nonmasculine character" (p. 2). By skillfully integrating historical and anthropological literature with the results of his own unique cross-cultural field work among contemporary berdaches, Williams provides an extraordinarily perceptive study of the berdache and the most comprehensive treatment of this controversial topic to date.

One of the goals of this work is to "allow Indian people to speak for themselves" (p. 7). The most effective use of quotations occurs in part 1, which explores the character of the berdache, including religious and ceremonial roles as well as the economic, social, and sexual aspects of being a berdache. Particularly valuable insights are conveyed by various traditionalists' translations of their own words for berdache: Cheyenne "halfman-halfwoman," Cree "man and woman," Lakota "two spirits, man and woman, combined into one spirit" (p. 85). Based on both written sources and his own empirical data, Williams concludes that spirituality, sex with men, androgyny, and woman's work are all indicators of berdache status and are all equally important.

Part 2 provides the historical perspective and examines postcontact changes in the berdache tradition, resulting from the devastating impact of colonization, missionary zeal, and government policies. The Spanish carried out their mission to suppress berdachism with especially notable vigor. Although the tradition still exists in parts of Latin America today, the religious element has been lost. In California missionaries eliminated berdachism among Mission Indians by 1820; Williams was unable to locate even a memory of the tradition during his field work. In the past several decades, the gay liberation movement has had a generally positive effect on berdachism in a two-way process of reinforcement.

Part 3—the briefest section in the book and the broadest in scope—places native American berdachism in a global context and investigates the nature of the female counterpart of the berdache (the "amazon"). Comparative data are drawn from past and present societies in which recognized gender categories approximate that of the berdache.

This is a provocative book that will undoubtedly rattle a number of cages. It will also prove to be an essential tool for scholars engaged in berdache or gender studies and an extremely useful source for ethnologists, historians, and others interested in the human condition. There is such a wealth of information in the book that some way might have been found to quantify it. Distribution maps would have been helpful. Graphics consist of sixteen plates; perhaps the most arresting is the famous 1830s sketch by George Catlin, "Dance to the Berdache."

MARJORY W. POWER
University of Vermont

SAMUEL J. WELLS and ROSEANNA TUBBY, editors. *After Removal: The Choctaw in Mississippi*. Jackson: University Press of Mississippi. 1986. Pp. x, 153. $22.50.

Following the Treaty of Dancing Rabbit Creek in 1830, the U.S. government applied a systematic policy of removal to the Choctaw, one of the larger and more advanced Indian tribes, residing primarily in Mississippi. The majority of the tribe, estimated at approximately 80 percent of the total population of twenty-five thousand grudgingly accepted their fate and moved from their ancestral homelands to Indian Territory. The remainder chose life on individual allotments, assimilation into the dominant white culture, or isolation in the Mississippi wilderness. Yet, the Mississippi Choctaws endured and now, as the Mississippi Band of Choctaws, have again secured recognition as a society.

Through the labors of scholars such as David W. Baird, Angie Debo, Arthur DeRosier, Michael Paul Rogin, Ronald Satz, Mary Young, and others, there is a significant body of literature regarding Choctaw removal and their history in Oklahoma. Little, however, with the exception of articles in state and regional journals and the unpublished works of graduate students, has appeared on Choctaws who remained in Mississippi. The work of a diversified team of authors and ably edited by Samuel J. Wells and Roseanna Tubby, the present book seeks to focus attention, through various topical essays, on that forgotten group of Choctaw people.

Satz gives a valuable survey of the subject from the removal period into the twentieth century. Abandoned by the government and besieged by settlers and land speculators, the Mississippi Choctaw "were victims of one of the most flagrant cases of fraud, intimidation and speculation in American history" (p. 8). Driven to marginal lands and doomed to a subsistence existence, the Choctaw adopted isolation as a means of survival. Toward the end of the century, however, attention was focused on the plight of the Mississippi Choctaw. The result

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was a reversal of government policy that eventually led to the organization of the Mississippi Band of Choctaws in 1945.

Clara Sue Kidwell, like Satz, offers an accurate and heartrending story of how the Mississippi Choctaw were deprived of their land, with particular attention to the role of the land speculator. And, using the writings of a historian and educator of the period, Henry Sale Halbert, Kidwell provides a most informative view of the social, economic, and religious life of the Mississippi Choctaw.

Three authors examine aspects of the Mississippi Choctaw population. Using the land and legal records of Clay, Oklibbeha, and Lowndes counties, attorney Rufus Ward concludes that the Choctaw, at the time of removal, enjoyed a life style comparable to that of white settlers. Wells, with land records and genealogical sources, studies specific members of the mixed-blood community and their actions. R. Halliburton, Jr., offers insights into one of the better-known and certainly more controversial mixed bloods, Greenwood Leflore, and especially his plantation, Malmaison.

Nineteenth-century religious leaders sought to address the needs of the Mississippi Choctaw. One such effort made by the Roman Catholic Church is discussed by John Christopher Langford, a veteran of ten years on the Mississippi Choctaw missionary field. From 1881, when Bishop Francis Janssens, the bishop at Natchez, opened Holy Rosary Indian Mission, until the present, the Catholic presence has sought to integrate the economic realities with cultural independence for the Mississippi Choctaw.

With the passage of additional Indian legislation and the effort by the Dawes Commission to isolate all Indians in Indian Territory, a second major program of Indian removal was launched in 1903 to encourage the Mississippi Choctaw to leave their homeland. Charles Roberts relates the activities of several religious groups, as well as Oklahoma Choctaw, to make the effort a success. With their numbers again depleted by removal, the native Choctaw once again retreated into isolation, where they remained until a federal agency was established among the Mississippi Choctaw in 1918.

The story of the modern Mississippi Choctaw, especially their economic development, is related by Jesse O. McKee and Steve Murray. Through cooperation with public and private groups and aggressive leadership, the Choctaw have made significant progress in health, education, and economic self-reliance.

With a good index, appropriate illustrations, and a useful bibliography, the book is technically well done and an excellent addition to the literature. Some will argue that the scope is misdirected, that omitted topics should have been addressed or that included topics could have been better developed. Yet the contributors deserve not criticism for what they failed to do but praise for their pioneering efforts. The book will hold the interest of the casual reader and should stimulate the serious scholar.

H. GLENN JORDAN
Northeast Louisiana University

JOSEPH BOSKIN. *Sambo: The Rise and Demise of an American Jester*. New York: Oxford University Press. 1986. Pp. ix, 252. $20.95.

The Sambo in this study is a close cousin but emphatically not the same character that appeared in Stanley Elkin's *Slavery* in 1959. Joseph Boskin treats him as "funny" in the sense of amusing and entertaining—a "comedic" type who became the "national jester" (pp. 155, 16). Yet, in claiming that Sambo became an entertaining American fool, Boskin also stresses that "Sambo was an overgrown child at heart," "given to impetuous play, humorous antics, docile energies, and uninhibited expressiveness" (p. 13).

For the most part, this study treats Sambo as a compelling fictive creation of the white man's inner necessities: "Sambo was an extraordinary type of social control, at once extremely subtle, devious, and encompassing." The aim was "to make the black male into an object of laughter . . . to strip him of masculinity, dignity, and self-possession" (pp. 13–14).

Much of this book presents interesting material on the rise of blackface minstrelsy after about 1830, the spread of the performing "Jim Crow, Esquire" to "Balls, Circuses, Theatres, Picnics, Churches, Schools, and Prisons" (p. 65). Even more fascinating is the fulsomeness of this tradition as it spread across the land after the American Civil War into such visual media as sheet music, joke books, advertisements, and postcards. The illustrations are compelling, especially those from the author's own collection, which includes a New Year's card with the enlightening dictum: "I'se mighty glad dat I can wish you all de good fings dis season."

Boskin casts a wide net, including such American icons as the Gold Dust Twins and the more enduring trio of Aunt Jemima, Uncle Ben, and the Cream of Wheat chef. The author's most compelling discussion is about radio and to a lesser extent television. He elucidates the subtle ways in which the Amos and Andy show (done, of course, by two white men) introduced less degrading formulas concerning blacks. Then, in an extended discussion, Boskin shows how Jack Benny's Rochester gradually departed further and further from the old stereotypes of his proper relationship with his white boss.

Unfortunately, the book has curious and unnecessary lapses. For example, in the early years of the

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Atlantic slave trade, "African chieftains" are said to have been "feted at European royal courts" (p. 19). Many readers will also be astonished by the assertion that "on the plantations, it was expected that one of the slaves' primary tasks was to entertain their whites" (p. 46).

A more difficult problem lies with chronology. The author claims that the Sambo stereotype began in the seventeenth century or even earlier. Surely this is too early. Most of the book's evidence suggests the first part of the nineteenth century, with a sudden blossoming about 1830. Yet the sudden emergence of Sambo is not explained. Rather, it is obfuscated by the astounding statement that Jim Crow "minstrelsy's dissemination of black culture heightened the struggle for emancipation" (p. 77).

Although this is a useful and interesting book, its concluding chapter, which discusses the demise of Sambo under a barrage of public humor by blacks that coincided with the civil rights movement of the 1950s and 1960s, leaves the reader wondering why—and indeed how fully—Sambo suddenly left the stage of American culture after such a prolonged and destructive performance.

WINTHROP D. JORDAN
University of Mississippi

JOHN DEMOS. *Past, Present, and Personal: The Family and Life Course in American History*. New York: Oxford University Press. 1986. Pp. xiii, 215. $17.95.

Although the history of the family invites an interdisciplinary approach, few historians of the family have pushed the limits of interdisciplinary inquiry as far as John Demos. In this collection of eight essays, Demos employs insights from psychology, demography, and anthropology to explore such issues as the changing nature of fatherhood, the history of child abuse, the rise and fall of adolescence, and the evolution of middle age and old age as stages of the life cycle. Many of the questions that Demos poses explore the dimly illuminated world of psychic and social experience in Puritan New England. How did people who have been dead for three hundred years feel? Did they love, ignore, or abuse their children? Did they experience storm and stress during adolescence, contentment in middle age, misery in old age? Demos is never content with a formal analysis of household and family size or birth and death rates. Rather, it is the history of the affections that rivets his attention.

The standards of proof in answering the questions that interest Demos are necessarily looser than in most other fields of historical inquiry. When not buried, the evidence is often conflicting. But Demos's ingenuity serves him well. He constructs plausible cases that child abuse did not exist in seventeenth-century New England and that Puritan old age, though socially honored, was psychologically vulnerable. The aged's "control of important resources seemed to command honor and respect, but not affection or sympathetic understanding" (p. 178).

Demos describes these essays as exercises in public history. Most originated in invitations to lecture before mixed audiences of psychologists, policy makers, businesspeople, and lawyers, and all relate to issues of concern to the helping professions. The latter, it appears, no longer need to be persuaded of the value of history, but can historians actually help the helping professions? In the final essay Demos describes his experiences as the house historian of the Carnegie Council on Children, which produced the controversial report *All Our Children: The American Family under Pressure* (1977). In retrospect Demos wonders whether historians have much to offer to such commissions. Necessarily remote from policy issues, the historian's "background presentation seemed vaguely comparable to the tunes piped into supermarkets and airline terminals" (p. 209). Relativists in their own disciplines, nonhistorians incline toward absolutism when it comes to history. They want clear answers of the sort that historians can rarely provide. Demos suggests that historians can act as gadflies in fields such as family policy, in which the line between scholarship and advocacy is often blurred. But gadflies, after all, are just flies. Historians can correct some of the more egregious assumptions about the past routinely entertained by nonhistorians, Demos notes, but he adds that, at the level of policy formation, it may not make a great deal of difference whether rates of child abuse or divorce were high or low in the past. Demos concludes in an upbeat but subjunctive mood: "Historical inquiry and policy formation make a new, awkward, and necessarily uncertain tandem. But practice may bring greater synchrony and increasingly substantial results" (p. 211). Then again, it may not.

JOSEPH F. KETT
University of Virginia

JUDITH WALZER LEAVITT. *Brought to Bed: Childbearing in America, 1750–1950*. New York: Oxford University Press. 1986. Pp. ix, 284. $21.95.

CATHERINE M. SCHOLTEN. *Childbearing in American Society, 1650–1850*. (American Social Experience, number 2.) New York: New York University Press. 1985. Pp. viii, 143. $22.50.

The genesis of Catherine M. Scholten's research on changing childbirth practices in the United States is recounted in *Childbearing in American Society, 1650–1850*. Unfortunately, Scholten's tragic death

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in 1981 prevented her from fully exploring the themes that are set forth in this short monograph. The author begins by contrasting childbirth practices in the colonial era, when the parturient woman was attended by a midwife and surrounded by female family members and friends, with the emergence of the medical management of childbirth that evolved in the late eighteenth and early nineteenth centuries as male physicians began to displace midwives as attendants at ordinary births. She attributes this transformation to a variety of causes including the formal medical education of physicians and the evolution of scientific obstetrics, the desire of women for safer childbirth, the utility of obstetrics in building up a physician's practice, and the many social changes associated with urbanization. Scholten argues that the displacement of midwives by physicians among urban, well-to-do women had, by 1825, wide-ranging social consequences, for "women no longer dominated the activities of the lying-in room" (p. 48). The contours of Scholten's award-winning article, "'On the Importance of the Obstetrik Art': Changing Customs of Childbirth in America, 1760 to 1825" (*William and Mary Quarterly* 34 [1977]: 426–45), are clearly reflected in this section of the book.

Scholten then shifts her attention from childbearing to child rearing, pointing out that the occupation of motherhood took on special meaning in the nineteenth century as the birth rate declined and child nurturing became an all-absorbing task. In the final pages of this work, she traces the reasons for the transfer of childbirth from the home to the hospital, a move that she depicts as "the most significant change in American customs of childbirth" between 1870 and 1920 (p. 106). The ideas presented in the second half of this book are both innovative and thought provoking, but Scholten's untimely death prevented her from examining them with the same thoroughness with which she investigated childbearing practices in colonial and early national America.

In her eagerly awaited work, *Brought to Bed*, Judith Walzer Leavitt also analyzes the changes that have taken place in American childbirth practices. Like Scholten, she describes how childbirth was transformed from "a woman-centered home event to a hospital-centered medical event" (p. 5). Yet there is a major difference in emphasis in these two works. Scholten attributes the end of women's dominance in the birthing room to the entry of male physicians into the lying-in chamber in the late eighteenth and early nineteenth centuries. Leavitt asserts that the sex of the birth attendant was not as crucial as the place of birth in determining who would control childbirth: she argues that "until childbirth moved to the hospital during the first third of the twentieth century, birthing women themselves were the most active agents of change in American childbirth history" (p. 4). In contrast to Scholten, who bases many of her findings on the prescriptive literature, such as textbooks and advice manuals that warned against the attendance of female friends and relatives in the birthing room, Leavitt examines the letters, diaries, and autobiographies of women and finds ample evidence that they negotiated with physicians about procedures to be used throughout the nineteenth and into the early years of the twentieth century.

Throughout this work Leavitt describes the combined efforts of women and physicians to provide for a safer maternity. After a detailed comparison of women's accounts with physicians' accounts, she determines that it was the congruence rather than the divergence of the two that was most striking. This seeming agreement may result in part from Leavitt's decision not to examine the midwife, a key figure in childbirth practices throughout much of American history. By excluding the ways in which physicians often used the midwife as a scapegoat for the high mortality rates associated with childbirth, however, she creates an artificial aura of agreement between the medical profession and parturient women. A serious question remains as to whether women agreed with doctors who characterized midwives as "ignorant" and "dirty," or whether they were culled into accepting the self-interested judgment of physicians without themselves having investigated the nature and role of midwives. When one examines the vociferous attacks on midwives that were engineered by obstetric specialists during the early years of the twentieth century, one can hardly conclude that altruism and a safer maternity were at the heart of the matter. Leavitt rightfully acknowledges that physicians supported the transfer of birth from the home to the hospital because they sought more power in the birthing room. But the full extent of this drive for power cannot be adequately understood without also recognizing the role played by physicians in bringing about the near demise of the midwife. Despite this qualification, Leavitt has written an exceptional book that broadens our understanding of the significance of childbirth in the lives of women and deepens our knowledge of how women have been active agents of change.

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MABEL COLLINS DONNELLY. *The American Victorian Woman: The Myth and the Reality*. Foreword by CAROL C. NADELSON. (Contributions in Women's Studies, number 71.) New York: Greenwood. 1986. Pp. xiv, 165. Cloth $29.95, paper $12.95.

ANNEGRET S. OGDEN. *The Great American Housewife: From Helpmate to Wage Earner, 1776–1986*. (Contri-

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butions in Women's Studies, number 61.) Westport, Conn.: Greenwood. 1986. Pp. xxiii, 256. $29.95.

The connection that has existed between women's history, women's studies, and the broader women's movement ever since the emergence of all three in the 1960s has consistently generated among scholars a concern for explicating women's situation to the general public. Thus, the writing of surveys of women's experience, of works based partially on primary scholarship but drawing heavily from secondary monographs, has become a major characteristic of the fields. When these sorts of works are well crafted they are of signal benefit: they bring together the findings of books devoted to narrow themes; they apply theoretical perspectives in new ways. Mary P. Ryan's *Womanhood in America: From Colonial Times to the Present* (1975) had such an impact, as did William H. Chafe's *The American Woman: Her Changing Social, Economic, and Political Roles, 1920–1970* (1972).

Both of the books under review fall into the generalist category. Both address broad interpretive subjects; neither's research base is exhaustive; and both are intended for the public as well as for scholars. Neither book, however, meets a criterion of excellence.

Beginning with the colonial period, Annegret S. Ogden argues that the definition of housewifery as an exacting series of tasks performed exclusively by women has not changed. Despite advances in household technology during the past two centuries, housework has expanded to fill all available time for its practitioners. Indeed, each age has added new responsibilities to homemaking without discarding the old ones. The eighteenth-century housewife was a manager and helpmate, whereas her nineteenth-century counterpart additionally had to be a lady—a model of refinement. The twentieth century added the roles of scientific expert, consumer specialist, and "supermom."

The thesis of the anomalous existence of a preindustrial gendered role for women in a postindustrial world is not new, although the book presents some interesting interpretations. Ogden nicely links the new domesticity of the 1920s to the power of the domestic science movement, and she effectively portrays the infantilization of wives by husbands in the 1950s. Still the book is beset by problems of analysis. Ogden consistently wanders beyond the realm of the home and thereby implies that the history of women is synonymous with the history of their domestic role: a dubious theoretical position even with extensive argumentation. Moreover, she delivers judgments with no apparent realization of their theoretical implications: she asserts with little proof that men suffered as severely from role conflict during the Victorian era as did women.

Mabel Collins Donnelly's book is beset by similar difficulties. In terms of interpretation, the book contains little beyond what is already available. Here we have the standard picture of strict socialization to femininity, of women's tendency toward illness, of the horrors of childbirth, and of all of this propelling women into reform activity. The existing literature has room for a synthesis of the many recent works on women and the Victorian era. But this book, with its failure to address major issues such as the role of religion in women's lives, is deficient. It suffers from a failure to consult some major recent works in women's history, a narrative approach that obscures the major theoretical issues of women's nineteenth-century experience, an episodic use of examples without any clear indication of why one is chosen above others, and a failure to understand the complexities of individual lives and attitudes.

LOIS W. BANNER
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KAREN BERGER MORELLO. *The Invisible Bar: The Woman Lawyer in America, 1638 to the Present*. New York: Random House. 1986. Pp. xv, 271. $19.95.

In the past ten years there has been a burgeoning interest in the history of women in the professions. Recent studies of medicine, science, nursing, and social work have documented the successes and tribulations of women who tried to enter professional life, establish institutional bases, develop successful career prospects, and obtain the appropriate opportunities and rewards for themselves and other women who might come after them. A history of women's experience in the legal profession has been sorely absent from this historical treatment, making Karen Berger Morello's study particularly welcome.

Morello uses a biographical approach to investigate the period from 1638 to the present. Through archival research and interviews with several hundred contemporary female attorneys, Morello tells the stories of scores of fascinating women. They range from the influential Margaret Brent, who became the executor of Maryland Governor Leonard Calvert's estate in the 1640s and the principal defendant in all lawsuits against the estate, to Justice Sandra Day O'Connor, an honors graduate of Stanford Law School who found that only one major California law firm would employ her after graduation and that that position was as a stenographer.

Morello captures the drama and frustration of the lives of these pioneering women. She recounts the prejudices they faced as well as the strategies they employed to overcome their exclusion and humiliation. The efforts of these attorneys to enter the profession were clearly linked to a larger commit-

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ment to equality for women in society. For example, Katherine R. Everett spoke for many of her colleagues when she recalled at age ninety-three that "what made me want to study law in the first place was seeing all these beautiful and talented women lawyers from New York marching for suffrage at President Woodrow Wilson's inauguration. They were so eloquent, so impressive, and they talked about using the law to deal with human rights and human problems" (p. 117). The links between the desire for professional equality and the struggle for reform resonate throughout many of these biographies.

Morello is to be particularly commended for including a chapter on black female lawyers in which she documents the burden of what she calls the "double impairment." Too many histories of the professions omit the important connections between race and gender. The portrait of Constance Baker Motley stands out as one of the most moving because it makes clear that the career of this outstanding lawyer and judge rested on a fortuitous meeting with a white philanthropist.

The author's background in law is particularly evident in her clear and cogent explanation of the struggle of recent feminists to compel major corporate law firms to hire women on equal terms with men. She is able to present their issues, strategies, and the outcomes deftly and clearly. But she is less interested than many historians of the professions in placing the book in its larger historical context. She does not address some of the recent debates about whether the professionalization project at the turn of the century enhanced or inhibited women's opportunities, nor does she compare her findings on the experience of women in law to the scholarship on other excluded groups, such as Jerold S. Auerbach's analysis of anti-Semitism in the Bar Association in his book *Unequal Justice* (1976). These and other questions on the history of the profession remain to be answered by future researchers. In the meantime as a result of assiduous research Morello has uncovered a hidden history and given us a compelling portrait of those who changed the definitions of who was eligible, able, and suitable to contribute to the practice of the law.

PENINA MIGDAL GLAZER
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National Inventory of Documentary Sources in the United States. Part 1, *Federal Records*; part 2, *Manuscript Division, Library of Congress*; part 3, *State Archives, Libraries, and Historical Societies*; part 4, *Academic Libraries and Other Repositories*. Teaneck, N.J.: Chadwyck-Healey. 1983–.

The cumbersome title of this huge undertaking does not give much information about its nature, although "national" is undoubtedly intended to state its scope. Chadwyck-Healey, the publisher and compiler of this reference work, has invited the major archival respositories in the United States to contribute the "finding lists" for their collections—registers, calendars, inventories, guides, and so on—to a "national inventory." This invitation, extended to "all important repositories in the United States," involves allowing representatives of the publisher to film (microfiche) finding aids selected by the repository. In addition to filming the aids themselves, the publisher is producing a master index to the aids. The resulting product aims to assist scholars in two ways: to help identify repositories containing materials relevant to a particular subject and to allow scholars to examine locally, at their own institutions, the complete finding aids for a collection.

The first of these aims—to identify repositories that may hold relevant documents unknown to the student or scholar working on a particular subject—is also the aim of some reference tools familiar to scholars: the *National Union Catalog of Manuscript Collections* (NUCMC), the collection holding listings in both the Online Computer Library Center (OCLC) and the Research Libraries Information Network (RLIN), as well as the printed book catalogues of very large important collections and repositories. There are several differences, however, in the intention and approach of these tools. The NUCMC, for example, may index an entire collection under the single personal name or topical subject of that collection. The aim of the *National Inventory of Documentary Sources* (NIDS) index is broader. It attempts, although wisely not claiming completeness, to describe in much greater detail the contents of a collection that may be described as "twelve boxes" and entered under one name in NUCMC. The publisher describes the "limitations" of the index thus: "The index makes no attempt to include every name and subject referred to in a finding aid. It is not practical or necessary to do so since the finding aids themselves are a form of index and are conveniently at hand." Although one could quarrel over what is "necessary" or at least useful, it is difficult to disagree with the assessment of what is "practical." Certainly, the degree to which NIDS achieves its purpose of identifying relevant repositories is to a large extent dependent on the comprehensiveness of the index. Evaluating that index—and evaluating the entire undertaking—is made difficult because the project is only in its beginning stages. It is not appropriate, therefore, to check the index and fault or praise it for its agreement or disagreement with what one may know of existing archival resources on a particular subject.

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A sampling of the index suggests the way in which it is being constructed. A search for additional repositories holding papers relevant to a minor nineteenth-century American political figure (whose principal papers are in the Library of Congress) yields two references. In checking the specific finding aids to which one is referred by the index entry, the politician is found (mentioned as a notable correspondent of the individual whose papers are described) in the narrative that precedes a contents list constructed along the familiar lines of "correspondence 1860–64," "financial records 1850–55," and so on. The usefulness of the index is thus dependent on the usefulness of those finding aids themselves and on how accurately and completely they describe a collection, as well as on how many of the names and subjects mentioned in a finding aid have been reproduced in the NIDS index. The resulting overall impression of the index will likely be one of unevenness in coverage and detail.

In the second of the aims of NIDS—to allow scholars to study the finding aids at their own institutions—it has no competition and serves a purpose that has long been identified as needed and important but that no one has undertaken until now. Faced with an entry in the NUCMC that a certain archive holds twelve boxes of materials on a subject and another archive three, the scholar's next step has been to seek out colleagues who have worked with the collection and rely on their memories and impressions of its scope or to call or write to the archivist of the collection. The opportunity to examine these finding aids oneself in order to assess the need to visit the archive and be better prepared for that visit is likely to be highly useful and valued by many. It is the major contribution of this undertaking.

NIDS has four parts, each of which can be purchased (or subscribed to) separately. Each has special features and could be reviewed separately. They are, briefly: part 1, Federal Records (including the National Archives, the presidential libraries, and the Smithsonian Institution); part 2, Manuscript Division, Library of Congress; part 3, State Historical Societies, State Libraries, and State Archives; part 4, Academic and Research Libraries and other Repositories. The first two parts can be purchased as completed works to which updates will be issued. They are accompanied by printed indexes. The second two parts are ongoing projects of so vast a scale that they will never be "complete." Each is issued in units, and each new unit is accompanied by a new, cumulative index (also on microfiche, of course).

As to completeness two important observations need to be made. In January 1987 Chadwyck-Healey reported that over two hundred libraries and archives had agreed to contribute to NIDS. This should be compared to the thirteen hundred repositories whose collections are listed in NUCMC. The second fact that must be considered is that not all the collections of those two hundred institutions are represented in this work. For example, the Lilly Library of Indiana University is listed by Chadwyck-Healey as one of the contributors to part 4. In checking the list of repositories where the summary collection names are listed under the name of the library, one finds only four of the Lilly's one thousand collections are included. That library is particularly strong in its collections of modern poetry, both in printed and manuscript sources. The library has chosen to contribute its finding aids to its extensive collection of papers of the American poet Sylvia Plath. To the researcher working on Plath this is an important aid. It does not, however, allow one to survey the holdings of the Lilly Library in the area of modern American poetry. Each library and archive has made a selection of the finding aids they wish to contribute, some a large number and some a much smaller selection of their complete holdings. This is, of course, the same assumption one must make of all similar cooperative ventures; even the mammoth (and closest to complete of all such records) *National Union Catalog, Pre-1956 Imprints* is not a "complete" record, nor does it allow one to review the complete holdings of the contributing libraries.

The extent to which NIDS is able to meet its expressed aim of covering "every repository that produces finding aids" will depend on the willingness of these repositories to cooperate with the venture and, from the publisher's point of view, the willingness of libraries financially to support the undertaking by purchasing and subscribing to it. (For parts 3 and 4—at a 1986 cost of approximately one thousand dollars per "unit," with five to six units published per year—a considerable investment is called for.)

The aims of NIDS are worthwhile and its promise great. John McCusker has contributed a very favorable assessment of NIDS to the publisher's brochure that accompanies the inventory. He speaks of the "great deal of time" he has spent "looking for (as opposed to looking at) appropriate records." The NIDS has the potential to save the time and effort of scholars: to offer considerable assistance in "looking for" and to both prepare and free valuable time for "looking at."

ANN BRISTOW
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BERNARD BAILYN. *The Peopling of British North America: An Introduction*. New York: Alfred A. Knopf, 1986. Pp. 177. $16.95.

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BERNARD BAILYN. *Voyagers to the West: A Passage in the Peopling of America on the Eve of the Revolution*. Assisted by BARBARA DEWOLFE. New York: Alfred A. Knopf. 1986. Pp. xxvii, 668. $30.00.

These two books contain the early results of Bernard Bailyn's ambitious, long-range project to trace the movement of people across the Atlantic to the British colonies in America. The shorter book presents a progress report that originated as three lectures, a familiar and valuable genre primarily designed for a general audience but useful to scholars and teachers as well. The longer and far more detailed volume forms, chronologically, the last of several projected volumes, this one on the English and Scottish migration to North America during the years following the Seven Years' War, especially 1773–74.

The first book cannot help but succeed. The intrinsic interest of the subject and Bailyn's intelligence and mastery of the language, combined with bold generalizations covering the whole sweep of the migration, make it ideal for students and stimulating for historians, even those who know the subject. The first and third chapters are especially successful. The first synthesizes the recent research proving the remarkable mobility of Europeans in the early modern period, and the third is a brilliant survey of the British colonies about 1700.

The core of *Voyagers to the West* consists of four chapters that discuss the wonderful "Register of Immigrants" for 1773–76. He has subjected the nine thousand people identified in the register to a detailed analysis, particularly of their occupations, motives, ages, sex, place of origin and of destination, and distribution by families. Numerous tables and graphs are accompanied by an interpretation drawing on literary sources, in which Bailyn emphasizes the skills of the emigrants (even of servants) and their division into two groups: one composed of families from northern England and Scotland, the other of single men, mostly indentured, from London and nearby counties. This extended discussion is preceded by a survey of the colonies receiving the immigrants and of British policy concerning the movement.

The following four chapters constitute a monograph that could stand by itself. Indeed, the entire book consists of a series of well-researched, interesting, and informative studies. This section, "Mobilizing a Labor Force," details the process by which British laborers were obtained to satisfy the colonial demand, especially in Pennsylvania, Maryland, and Virginia; the shipment of these rather reluctant, young, very poor, mostly male emigrants; and their sale and purchase, all good economic and social history. We do not, however, learn their ultimate fate.

The closing chapters, on the other hand, explicitly deal with nearly a dozen quite different attempts at colonization, mostly by families from northern England and Scotland, caught up in speculative attempts to populate relatively unoccupied land along the edges of settlement from west Florida to Nova Scotia. Each makes excellent reading, but most of the schemes failed, and, ultimately, the reader wonders about their significance. This volume seems less integrated and lacks the interpretative impact of Bailyn's shorter book.

The two books suggest several questions. The first concerns whether the characteristics of the emigrants changed over time. Bailyn stresses the superior quality and skill of the 1773–75 group. The early colonists, however, also included a large proportion of families, some with substantial properties, and many seventeenth-century servants claimed a skill, so that one questions Bailyn's conviction that his late colonial sample represented a marked improvement.

Second, the word "peopling" suggests a process by which immigrants filled an empty land. Such was not the case during the years 1773–75. Those on the register who did not die or leave joined over two million established colonials, who every year were adding to their number four or five times as many individuals as the new arrivals. Moreover, even the "peripheries" were not vacant but were home to thousands of Indians and Acadians, who were being invaded by tens of thousands of settlers themselves on the move.

Finally, one wonders about the importance of the single men, who formed such a large proportion of the immigrants throughout the colonial period. Were they really skilled? Were they needed? New England got along quite well with only a few of them. Their contribution may have been only to help kill off Indians and enrich their white masters. Did they "people" the land? Women, and the families they helped found, did that, and one questions whether historians should concentrate on them and on the men they married rather than on the hapless, ephemeral servants and laborers.

One does not read books for their imperfections, or we would all write in vain. Both of these works are well worth reading, informative, indeed fascinating, and one eagerly looks forward to their successors.

JACKSON TURNER MAIN
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DIANA WILLIAMS COMBS. *Early Gravestone Art in Georgia and South Carolina*. Athens: University of Georgia Press. 1986. Pp. xiv, 246. $35.00.

Twenty years have passed since Allan I. Ludwig published his pathbreaking study of early New En-

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gland gravestones, *Graven Images: New England Stonecarving and Its Symbols, 1650–1815*. Ludwig's book was the first major work on the subject in nearly one-half century, but it soon was followed by several important books and numerous dissertations and scholarly articles on gravestone art in seventeenth- and eighteenth-century America. For some reason, doubtless to be examined in detail by future historians, the American 1970s found the subject of death and dying fascinating in the extreme. Thus, while the tabloids were filled with pseudoscientific reports of people returning from the abyss with tales of life on the other side, many historians (this one included) found themselves puttering around in old graveyards and poring over ancient funeral sermons.

As is so often the case in American historical studies, works on New England have always dominated this field to the point of near exclusion. Recent work has been no exception. But, now that the present generation's popular fascination with the macabre has faded somewhat, Diana Williams Combs has produced a handsome volume that seeks to correct the traditional regional imbalance.

Combs demonstrates in her book that most of the early iconic tombstones still extant in Georgia and South Carolina (many were destroyed in the Revolutionary and Civil wars) actually were the work of New England carvers. Indeed, Combs points out, in what is her book's most noteworthy discovery, that many of the most elaborate and sophisticated works of the New England carvers cannot be found in their own burial grounds but instead were placed in southern cemeteries where their particular motifs were greeted by a more hospitable regional culture. In addition, a good deal of European innovation in stone carving during this time found its way to America not through New England but through the relatively cosmopolitan cultural community of Charleston. Among other things, the discovery, in Combs's words, that "the relationship between the [New England] gravestone carver and his craft did not necessarily reflect a direct correlation between the carver's own religious convictions and those of the community in which his gravestones appear" (p. 3) serves as an important reminder to historians in other fields: when working with sources produced for commercial exchange, attribution of artistic (or social or psychological) intent can be a very risky business.

The most satisfying work in this book is the detail. Combs knows tombstones. Considerably less satisfying are her efforts at larger cultural analysis. In the first place, she is either stingy in acknowledging or ignorant of the work of others in this field who preceded her with at least equally original work. For instance, she grants the status of previous "major studies" only to Ludwig's book and to Dickran Tashjian and Ann Tashjian's *Memorials for Children of Change* (1974), thus minimizing or altogether ignoring important studies by Peter Benes, James Deetz, Edwin Dethlefsen, Maris Vinovskis, Stephen C. Foster, and others—including the well-known, though unpublished, work of Patrick N. Butler on Tidewater Virginia. (Except for Benes, whose very influential book, *The Masks of Orthodoxy* [1977], is mentioned only in a grudging and critical footnote, none of these authors, all of whom are in the forefront of such scholarship, appear anywhere in Combs's study.) Moreover, having created an impression that her work is conceptually more pioneering than it in fact is, Combs doubly disappoints in her closing chapter by deriving her own larger analysis directly from such works as Ann Douglas's *The Feminization of American Culture* (1977) and Mary P. Ryan's "American Society and the Cult of Domesticity" (1971), works of undeniable originality in their time, but hardly recent offerings.

In sum, this book will be valuable to historians interested in what its title accurately and modestly promises, early gravestone art in Georgia and South Carolina. Any suggestions contained in the body of the book that it does more than this may safely be ignored.

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WAYNE CRAVEN. *Colonial American Portraiture: The Economic, Religious, Social, Cultural, Philosophical, Scientific, and Aesthetic Foundations*. New York: Cambridge University Press. 1986. Pp. xx, 459. $39.50.

Wayne Craven has undertaken the seemingly impossible task of writing a comprehensive account of colonial American portraiture from its beginnings until shortly after the revolution, and, as his title promises, he has attempted to ground the study in "economic, religious, social, cultural, philosophical, scientific, and aesthetic foundations." Such lofty ambitions leave themselves open for criticism because no one individual—despite his excellent qualifications and thorough grasp of the factual material—can cover so much territory. Thus, Craven's long-awaited book is a montage of artists and their sitters framed beneath a veneer of historical data. Despite the necessary superficiality of such a broad survey, the results are solid and often quite interesting.

Craven has, in fact, made a significant contribution to the history of colonial American art by pulling together a storehouse of information that has heretofore lain buried in journals, exhibition catalogues, and monographs. The author does not attempt to trace obscure attributions, discover unknown artists, or startle us with unexpected find-

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ings; rather, he has synthesized existing material and ordered a historical structure for considering colonial portraiture. The book is divided into three sections, each centered on a pivotal thinker of the time: John Calvin for the early period, Cotton Mather for the middle years, and Benjamin Franklin for the revolutionary era. Within this framework Craven tries to equate the evolution of an indigenous style of American portraiture with the growth of mercantilism, the diminishing influence of austere Puritanism, and the obvious signs of an increasingly secular society. Each section is introduced by a few chapters designed to provide the historical and intellectual background and set the stage for the art work created at the time. Although on occasion Craven relates social customs—the length of male hair, for example—to observations about conventions in portraiture, such connections are tenuous and not altogether convincing. Yet one is somehow left wanting more direct links between the wealth of factual information and the individual portraits. Concentration on a few specific works might have given added dimension to his premise that changes in portraiture style evolved as a result of religious and social developments. Instead, Craven has chosen to assemble vignettes about the somber ladies and gentlemen who peer at us from the dusky canvases. Little-known facts about the Freakes, Pierponts, Byrds, Bollings, Bowdoins, Lees, and many other colonial families anchor a number of familiar faces with the economic, social, and political events taking place at the time.

One of the book's strongest and most original chapters deals with the English art manuals by Henry Peacham, John White, and William Salmon that served as "how-to" guides for the pragmatic colonials who found little helpful instruction in the erudite theoretical treatises of Charles du Fresnoy and Roger de Piles. Summaries of the careers of well-known artists from Peter Pelham to Charles Willson Peale are also quite valuable as are separate discussions of portraiture in Virginia, Maryland, and South Carolina. But Craven's survey of the regions only skims over the rich and unique period of portrait painting in the Hudson Valley and virtually ignores the German-American artists of Pennsylvania. These, however, are small weaknesses in a comprehensive book that fills a gap in the history of American art.

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DANIEL R. COQUILLETTE *et al.*, editors. *Law in Colonial Massachusetts, 1630–1800*. Boston: Colonial Society of Massachusetts; distributed by University Press of Virginia, Charlottesville. 1984. Pp. lxviii, 608. $30.00.

Both scholars and beginning students of legal history will find this book essential. Scholars will discover a wide range of essays by prominent historians. They add in particular to our knowledge of the practice of law, although several discuss specific areas of the law and one, legal literature. Beginning researchers will learn from these, of course, but for them the volume contains an even more important contribution. Eight of the nineteen essays focus on sources and the use of court records in conducting research.

Legal historians continue to view the early national period as the "formative era" of American law yet regard colonial law as unlearned, unsystematic, and largely irrelevant to later developments. This collection of essays will help dispel those long-held beliefs. In detailed studies of legal practitioners in seventeenth- and eighteenth-century Massachusetts, Thomas G. Barnes, George Haskins, Barbara A. Black, Russell K. Osgood, Neal W. Allen, Jr., Charles R. McKirdy, and Daniel R. Coquillette attack the myth of ignorance and informality among colonial lawyers and judges. As early as the mid-seventeenth century jurists possessed learning (if not necessarily in the law), skill, and commitment to professional standards. Although John Adams railed against "pettifoggers," Coquillette points out, he also associated with a well-established group of erudite men, learned in the law and legal theory. The earliest "lawyering" (p. 3) in Puritan America, Barnes explains, flowed from the pen of Thomas Lechford, who earned a good income producing a wide assortment of legal documents. ("The range is staggering . . . The volume is no less remarkable" [p. 21].) Apparently, New Englanders had as much need as their more sedentary contemporaries for the services of a competent legal draftsman and scrivener. On this point we must remember that a good deal of colonial law depended on the form in which writs, pleadings, conveyances, and so on appeared. Lechford was not simply a scrivener; in creating legal documents, he practiced law.

In their discussions of the careers of two men active at the end of the seventeenth century, Black and Osgood demonstrate the range of activity that existed in colonial Massachusetts law. Nathaniel Byfield (1653–1733) and his New World peers at the top of the professional ladder understood English legal practices; their deviations from those standards were made by choice, not ignorance. Osgood's close investigation of the judicial book of justice of the peace John Clark (1667–1728) demonstrates that, although Clark was not trained in the law, he handled the legal problems of his neighbors in a manner remarkably similar to his peers in England.

Although these and the other articles on legal practitioners add to our understanding of colonial law, I wish less emphasis had been placed on indi-

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viduals and more on the nature of the law they practiced. The portions of the studies that focus on what was done, rather than on the expertise of those who did it, are most satisfying. Unfortunately, they are not central to the concerns of a number of the authors. In this regard, the pieces by Douglass Lamar Jones, David H. Flaherty, and William E. Nelson are particularly useful for they analyze the kind and quality of law practiced in early Massachusetts. Jones's investigation of the law of poverty offers a perceptive analysis of changing attitudes toward the poor and the obligation of the community to take care of them. Flaherty discusses criminal law in provincial Massachusetts, focusing on the availability of defense counsel and the nature of the defenses used. Nelson well demonstrates the relationship between colonial experience and that of the nineteenth century in a study of conflict of laws and the evolution of federalism. He believes that the U.S. Supreme Court and federal law were designed to play a role similar to the Court of King's Bench and the common law of England. Thus, the federal court system was both an innovation and the result of the colonial past.

The last two sections of the book contain articles by law librarians and archivists as well as historians. They explicate in a clear manner legal terminology, the organization of various kinds of court records, the structure of the courts, and rules of pleading and procedure. In addition, Michael S. Hindus has provided a summary inventory of county court records through 1859. These valuable articles will save graduate students and other beginning researchers in legal history countless hours of effort. Massachusetts has long been known as a careful preserver of its historical records. Archival guides such as the ones presented here will encourage historians to use the available documents. As a result, we will continue to see an emphasis on Massachusetts in the history of colonial law. One wishes that the records of other states were in such capable and energetic hands.

MARYLYNN SALMON
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ROGER THOMPSON. *Sex in Middlesex: Popular Mores in a Massachusetts County, 1649–1699*. Foreword by DAVID D. HALL. Amherst: University of Massachusetts Press. 1986. Pp. xix, 252. $30.00.

"Concerned, considerate, and cooperative" citizens lived in late seventeenth-century Middlesex County, Massachusetts, according to Roger Thompson, a historian who detests the "brutal, suspicious, intolerant, and bigoted" image fostered by those he dubs feminists, psychohistorians, and anti-Puritans (pp. 199–200). Thompson concludes that court cases—his virtually exclusive source—affirm that "tenderness, love, care, [and] helpfulness" were generously heaped on servants, children, and spouses (p. 196). He also contends that Puritans were not "squeamish" about sexuality and that no "sub-culture of the bastardy-prone" existed as youngsters rich and poor responded to awakening hormones by "sowing their wild oats" before settling down to the serious business of being good Puritans (pp. 94, 99). Notwithstanding a vibrant presentation of cases, however, Thompson fails to support his major contentions and often interprets evidence in a highly imaginative way.

From instances of young swains uttering words of love to seduce otherwise resistant lasses and of friends of abusive husbands providing testimony about those defendants' "tender" behavior, Thompson draws generalizations about Puritan caring in a broad sense. He seems not to realize that love talk could serve the purposes of exploitation rather than tender partnership. Moreover, he provides more illustrations of men with erections simply approaching women or reaching up under their coats than he does of gentle busses. Those tender partners that he holds up as examples—Anne Bradstreet, Anne Hutchinson, and Ann Hibbins—were all unusual women, and the latter two were even chastised for overriding their husband's authority. The tender treatment of servants and children is not supported by any concrete evidence at all. (The fact that certain witnesses were appalled about specific instances of patriarchal cruelty reveals nothing about its occurrence.)

Although the court records do not reveal much fornication, Thompson asserts that an "estimated one hundred adolescents" frolicked and "were resolutely interested . . . in sex," but many of the frolics he describes involved music, theft, laughter, and nightwalking, without reference to sex (p. 89). His one hundred adolescents represent—at best—only 1 percent of the total half-century's population. Since he cannot identify most of these youths by name, no proof is supplied that these one hundred became good Puritans as their alleged ardency waned. Although Thompson chides the idea of a subculture of bastardy, Eli Faber's statistical study of Middlesex County over roughly the same period reveals that nearly 40 percent of all fornication cases involved servants. Thompson, in contrast, cites no statistics.

Thompson's reluctance to use statistics again damages his case for asserting these New Englanders were not "squeamish" about sex, because he cites instances of bawdy talk and behavior at the same time that he indicates that a good deal of modesty existed. The reader is left wondering which attitude was more common and whether those persons who spoke explicitly were, in fact, churchmembers. And

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Thompson must explain the one case in which a woman who spoke graphically of a man's sexual overtures was herself admonished for "bold familiarity" (pp. 130–32). The author also maintains that a wife could demand sexual satisfaction from her husband, but in the one case he presents of a woman doing so, there is no proof offered that the community sympathized with her.

Thompson further assumes that because certain motivations and behaviors are not reflected in the court records they could not have existed. He uses negative evidence to maintain that couples did not attempt, through sex, to force parents into approving of a marriage and that women did not try to hook a husband thereby, but such motivations would not be readily admitted. Then, in his discussion of masturbation as a mere "trick of youth," Thompson uses negative evidence to conclude just the reverse. Apparently, Thompson's beliefs about what must have happened take precedence over anything else.

Thompson also appears to have some animus against women. He refers to women in their twenties as girls but to no such males as boys. He attributes rape to "resolutely erotic" young men, ignoring the fact that contemporary professional thought views it as an effort not to relieve lust but to dominate women. He continually stereotypes women as "disturbed," "melancholic," self-appointed moral monitors, and "faithless huzzies," whereas he is much less likely to stereotype men. Adulterous husbands are sometimes "sex-starved," implying that the fault rested with their wives. Pregnant women frequently lie about their sexual experience in court because, Thompson fancifully asserts, it takes, on the average, fifty copulatory acts to achieve pregnancy. The evidence for such conclusions and character typologies is minimal, if not totally absent.

Yet, although he draws distinctions between his male and female subjects, the author nevertheless affirms that women were not "subordinate and powerless" because they had "important official functions as midwives," spoke up in court, chose their own mates, and expected sexual satisfaction (p. 196). This seems very little to base such a conclusion on, and no real evidence is provided for the latter two claims. The facts that Thompson can supply so little information about the "weaker sex" and chooses to ignore feminist descriptions of the dimensions of power in Puritan society reinforce his apparent bias not to take women very seriously.

In essence, this book does not add anything new to our understanding of these early Americans. Thompson's neglect of statistical proof, his reluctance to use the contrary findings of other scholars, and his impressionistic interpretations make this a work of fiction, not fact.

LYLE KOEHLER
New Mexico State University

PAUL J. KORSHIN, editor. *The American Revolution and Eighteenth-Century Culture: Essays from the 1976 Bicentennial Conference of the American Society for Eighteenth Century Studies.* (AMS Studies in the Eighteenth Century, number 5.) New York: AMS Press. 1986. Pp. xiv, 271. $39.50.

This book is a collection of (apparently) revised essays originally delivered at a 1976 conference of the American Society for Eighteenth-Century Studies. There is little unity among the separate pieces, apart from the fact that all deal with some aspect of the eighteenth century (and one article by Roger T. Anstey fails to meet even this criterion). In his introduction Paul J. Korshin argues that the essays "fall into two clearly separate groups, those that deal with aspects of politics and freedom and those that cover the emergence of new literary and artistic ides [*sic*] in the world around the American Revolution" (p. ix). True enough, but even when placed in these groupings the individual essays are disjointed and uneven.

The book begins with an essay by M. M. Goldsmith on the decline and fall of Sir Robert Walpole, which concludes that the ideological consequences for British politics were "disillusionment, cynicism, and skepticism" (p. 22). This is followed by a long, detailed essay by Jeffrey Barnouw that critiques the "Atlantic Republican" thesis presented in J. G. A. Pocock's *The Machiavellian Moment* (1975). Isaac Kramnick follows with a brief study of Tom Paine that seeks to place his later political writings in the context of his English working-class background (themes presented here are dealt with at greater length in Eric Foner's *Tom Paine and Revolutionary America* [1976]). Anstey next presents a study of British abolitionism that concentrates mostly on the nineteenth century. The first section concludes with an analysis by Donald J. Greene of the "rhetoric of liberty" in mid-eighteenth-century Britain.

The second group of essays begins with a study by Irma Jaffe of ethics and aesthetics in eighteenth-century American art (which is chiefly an analysis of John Singleton Copley's "Watson and the Shark"). This is followed by what can only be termed a perverse study by Karl Keller of "literary excess" in eighteenth-century American authors (which again strays beyond the bounds of that century to include Edward Taylor, Jonathan Edwards, and James Fenimore Cooper). This in turn is followed by an essay by Kenneth Silverman reviewing economic arguments for and against the theater in colonial

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and revolutionary America. The book concludes with a summary essay by Durand Echeverria on images of American society held by French intellectuals before 1815.

To the obvious question "why are these essays appearing in print so long after they were delivered," the editor notes that the American Society for Eighteenth-Century Studies had contracted to reprint the essays but that the society "lost its willingness—and forgot its contract—shortly after the Bicentennial celebrations were over" (p. x). This may explain why several of the authors, in their notes, cite other of their writings published after 1976. Barnouw, for example, cites other articles he published in 1980 and 1983 as well as a 1981 paper "being revised for publication." Other authors (Anstey, Silverman) had just published books that treat the themes of their essays in greater detail. And one essay (by Echeverria) was actually published in identical format in the *French-American Review* in 1982! The net result is to suggest that the essays in 1986 are not exactly on the cutting edge of historical research, whatever their status was in 1976.

Besides this basic drawback, the printing of the book is shameful. If we must wait so long for the essays to be printed, they ought at least to be presented in an accurate and attractive format. Such is not the case. The book abounds with typographical and printing errors. Typical examples (selected at random) are "affaris" for "affairs" (p. 6), "many" for "may" (p. 11), "have" for "having" (p. 13), "dicussion" for "discussion" (p. 76), "floursiehd" for "flourished" (p. 78)—the list could continue almost indefinitely. Discovering that two of the first three pages in the introduction are printed backward does not inspire one with confidence (pp. ix–x). Sloppy editing sometimes results in the height of absurdity, as when we are informed that Paine "was not the notorious author of the godless *Age of Reason*"—"not" is printed instead of "now" (p. 88). One gets a sense that, after all these years, the essays were thrown together almost as an afterthought.

There is little reason for the serious scholar to consult this book. Given the eminence of the contributors, the volume cannot be considered a consequence of the "publish or perish" syndrome, but there is not much here that is not available elsewhere, and with more detail and sophistication. This book confirms the cliché, "too little, too late."

JOHN F. BERENS
Northern Michigan University

HENRY MAYER. *A Son of Thunder: Patrick Henry and the American Republic*. New York: Franklin Watts. 1986. Pp. xvii, 529. $22.50.

Henry Mayer's book is a traditional, some might say old-fashioned, political biography. The volume tells a story: a story of Patrick Henry, of the Virginia in which he lived, and of the political battles that shaped his life and that, in turn, helped shape the life of a new nation.

The author tells us in his preface that he intends to re-create the political atmosphere in which his subject moved, and that is precisely what he has done. Traveling with knowledge and authority through Virginia politics, Mayer manages to convey a sense of time and place that is unusual in much contemporary historical writing. At times Henry is shunted to the background as Mayer introduces us to a tavern discussion of the John Chiswell murder case or seats us at the First Continental Congress as the delegates debate the adoption of a nonconsumption agreement.

Mayer has not, as he is the first to admit, unearthed a "cache of hitherto unexplored evidence." There is "no secret diary, no dusky mistress, nor even a rough draft of the 'liberty or death' speech" (p. xv). The author presents no radical new interpretation of Henry or of Virginia politics, nor does he suggest barely plausible psychological explanations for the events of the time. This is not a volume for the "expert" unless one simply wants a good read.

What Mayer has accomplished is the writing of a readable and attractive biography for the general reader or the historian seeking a general knowledge of the subject. The author is thoroughly familiar with the primary sources on his subject and is equally knowledgeable about the secondary literature in the field. His descriptions and conclusions are accurate, his interpretations plausible, and his narrative style outstanding.

Too many disciplines have severed their connections with the reading public, and it has too often been assumed that scholars speak only to each other. In Mayer's biography of Patrick Henry we have a competent piece of work that combines a high standard of scholarship with an understanding that history, to be read, must be readable.

DAVID L. AMMERMAN
Florida State University

ROBERT P. DAVIDOW, editor. *Natural Rights and Natural Law: The Legacy of George Mason*. (George Mason Lectures.) Fairfax, Va.: George Mason University Press; distributed by University Publishing Associates, Lanham, Md. 1986. Pp. v, 264. Cloth $18.75, paper $12.50.

As a historic figure associated with our national origins, George Mason is accorded little space in most textbooks, and, compared to his contemporary

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Thomas Jefferson, he is outweighed in the bulk of biographies by perhaps a 200 to 1 ratio. Confronted with this cumbersome fact, Virginians have tried to make amends by naming a university after Mason and subsidizing his home as a tourist attraction. From the university a series of constitutional symposia have emerged, each intended to produce a volume that would call attention to Mason's part in events from 1774 to 1788, a time when (as Jefferson said) Mason displayed qualities "of the first order of greatness."

These essays from a symposium held in 1984 will do little to advance Mason's reputation as a Founding Father but may help the dialogue probing the origins of political experimentation. Mason wrote the Virginia Constitution of 1776 and was an active debater at the Federal Convention in 1787. Neither fact, incidentally, is mentioned in three of the four essays.

Editor Robert P. Davidow has endeavored to cover the four topics of the essays under an umbrella created by Mason's Virginia Declaration of Rights, which called for "frequent recurrence to fundamental principles." Blessed by this vague ambiguity, Davidow points out that the ideas of Mason's generation are discussed most directly in John Murrin's article, "Can Liberals Be Patriots?" In fact, that is the only essay that deals with Mason's ideas in any historical sense. The other three are well-reasoned excursions into etymology by professors of religion (David Little and James Childress) and law (Michael J. Perry). Only historian Murrin stuck to the guidelines, it seems, and he reminds the wordsmiths: "Even though the language of the founders can seem deceptively modern on commonplace topics, we should expect it to convey presuppositions about life and society that we no longer share and that we can understand and appreciate only through patient effort" (p. 51).

Murrin is, of course, driving at the controversy over activism in our time. After Murrin's lucid essay (with the answer to the question in the title, yes, liberals can be patriots but "they could often sound more liberal than they were"), much of the three remaining articles consists of efforts to strike a blow for or against so-called judicial activism. Little initially focuses on Mason's Virginia Declaration of Rights then gives a lengthy comparison of John Locke, Karl Marx, and texts from officials of the Reagan administration. The natural rights theories have worked themselves into modern society until now "all peoples and governments are . . . subject to international scrutiny," he writes, and must demonstrate a commitment to human rights or explain their actions. Childress is concerned with the natural law as it bears on "the right to life" (euthanasia, abortion, and so on) and discusses medical ethics in the context of a modern morality as practiced (or not) by doctors and theologians. Charitably, he does not rhetorically ask: "What would George Mason decide in a delivery room, when a baby suffering from Down's syndrome was born?"

Lawyers are torn over the current fuss about the intentions of the Founding Fathers in 1787, as Perry acknowledges in his defense of "nonoriginal interpretation by the federal judiciary against the other branches of the federal government" (p. 207). A "serious disarray of political-moral philosophy" exists, Perry says, and he pleads with those who stress the framers' "intent" to "tell us why the text thus understood should be authoritative. It will not do . . . to say that the text thus understood should be authoritative because the ratifiers wanted it that way" (p. 215).

Given the quality of the participants in this symposium, the essays are not a disappointment. Rather, they are contributions to the dialogue of dissent that did not start in 1776 and will never end, as long as the "fundamental principles" of Mason are observed in the United States. More emphasis on Mason's role in creating the intellectual climate for these clashes of opinion would have been helpful to those of us who agree with Yale President Benno C. Schmidt's view of the original intent versus activist debate. The rigidity of the "intent" crowd is improper, Schmidt said recently, because "history is not so simple and not so static." A short point sometimes does not require a long essay.

ROBERT A. RUTLAND
University of Tulsa

BROOKE HINDLE and STEVEN LUBAR. *Engines of Change: The American Industrial Revolution, 1790–1860*. Washington, D.C.: Smithsonian Institution Press. 1986. Pp. 309. Cloth $29.95, paper $14.95.

A major new exhibition, "Engines of Change," opened at the National Museum of American History in Washington, D.C., in November 1986. Two Smithsonian publications bear the same title (the other, shorter one is by Steven Lubar alone), and both function as exhibit catalogues. But the present book by Lubar and Brooke Hindle certainly stands on its own as more than a reminder of what the exhibition shows. That "more" is difficult to characterize. It is not another coffee-table book, although its black-and-white illustrations are profuse and attractive. The text provides a serious and readable exposition of some major technological changes in antebellum America and their social effects, and it laudably avoids the heroic-inventor mode of explanation for those changes. It integrates social, economic, and cultural considerations with technology

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and offers much information beyond the exhibition itself about nineteenth-century American industrialization.

Yet the book explicitly derives from the exhibition of "physical survivals of technologies of that era" and "endeavors to look through these artifacts to gain an understanding of the Industrial Revolution." So "the photographs, drawings, and maps included here . . . in some ways . . . constitute the book's primary message" (p. 7). Unfortunately, however, that visual message is insufficiently provided with verbal explanation. With a few welcome exceptions, the figure captions are overly laconic. "A mill girl at her loom" (p. 198) and "View of canal lock" (p. 115), for instance, do not explain themselves, nor does the text identify them. Illustration credits tell who now owns an image but only rarely where it originated. The diagrams of machines, steam engines, water wheels, and turbines, lacking verbs to say what the parts do, are likely only to frustrate a reader who attempts to learn from them. (Lubar's shorter catalogue better addresses the questions of "how did it work?")

Perhaps because of the book's orientation to the exhibited objects, its coverage of topics is uneven: railroads get twenty-six pages, iron and coal only three. Careless editing in some details has left, for instance, a spinning wheel and spinning jenny confused (p. 22), muskets as rifles (p. 228), and New Lanark in England instead of Scotland (p. 237). Nice bibliographical essays for each chapter help compensate for the skimpy footnotes. A good history of nineteenth-century American technological change is sorely needed for classroom purposes; with caution and supplementary materials an instructor might well use this one.

CAROLYN C. COOPER
Yale University

CHARLOTTE M. PORTER. *The Eagle's Nest: Natural History and American Ideas, 1812–1842*. (History of American Science and Technology Series.) University: University of Alabama Press. 1986. Pp. xii, 251. $24.95.

This book's title asserts that its contents will address the relationship between natural history and "American ideas." This is a noble intent, given the need for intellectual history to define cultural contexts and the problem that the history of American science has become befogged with attention to petty details buried in often trivial manuscript sources.

Unfortunately, Charlotte M. Porter's book does not fulfill the promise of its title nor advance the conceptualization of the field. It is never clear why this period connotes a unique subject matter. The book lacks a central, overarching thesis to interpret the events of that time span, and the material treated fails to prove that American naturalists bore a significant and revealing relationship to "ideas." Buried somewhere in the author's intent are the attraction of the idea of the inferiority of American flora and fauna to the natural history of old Europe and the parallel concept that this notion served to explain the careers and enthusiasms of early nineteenth-century American naturalists. These men, such as Constantine Rafinesque or Alexander Wilson, who strode singly through the wilderness, are then placed in antipodal mode to the rise of professionalism ("peer review"). Thus, the activities of such as Thomas Say, the Peales, William Maclure, and the New Harmony scientists are seen in contrast to the new institutionalism represented by the hard new science propounded by men of the class of the botanist Asa Gray. (James Dwight Dana, Joseph Leidy, James Hall, or Louis Agassiz would have been equal if not superior comparative figures.) Again, this is an interesting idea, but it remains just that, and the striking individuals absorbed in the romance of natural history are described in pancake-flat fashion that denudes them of significance or individuality. The book's laundry list of scientists and the institutions they inhabited have all been set forth and skillfully drawn in the biographies, institutional histories, and special studies that mark the solid work of the leading authorities in the field who identified its contours in recent decades.

Porter's book, therefore, is too thin to be a synthesis and lacks the originality of conception that justifies its claim to attention. But for the useful description of naturalists associated with Philadelphia's Academy of Natural Sciences in the early decades of the nineteenth century, one wonders why the book was written. The bibliography lists thirty manuscript collections consulted. One has to wonder why young scholars are forced or feel compelled to engage in this drudgery, when little results to enliven the people and institutions that made the subject professionally engaging in the first place. The writing is often stilted and abounds with such trivia as "David Owen married Caroline Neef, the daughter of Maclure's Pestalozzian teacher, and shared their interests to the extent that he was willing to perform a postmortem dissection of his father-in-law's brain" (pp. 113–14). As the intellectual enthusiasms of yesterday become the doctoral dissertations and university press monographs of today, it is all the more important that such productions be subject to rigorous "peer review." This book and its author were not served by this process, despite the praises bestowed by dust-jacket blurb writers.

EDWARD LURIE
University of Delaware

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H. LARRY INGLE. *Quakers in Conflict: The Hicksite Reformation*. Knoxville: University of Tennessee Press. 1986. Pp. xv, 310.

In April 1827 the Hicksite Quakers of Philadelphia Yearly Meeting claimed irreconcilable differences with the Orthodox hierarchy and decided to establish separate meetings. Although the Society of Friends had weathered a number of internal controversies since its beginning around 1650 in England—disputes over the wearing of hats during prayer, the authority of meetings for discipline, participation by Friends in civil government, and slavery—this time the society split apart. In 1827 theology and authority were at stake. The Orthodox were evangelicals who, like those of other denominations, were trinitarian, placed great emphasis on salvation through Christ's sacrifice, and encouraged Bible study as the way to know God. Although the Hicksites did not entirely reject the historical Jesus, they held that traditional Quakerism centered on the light of Christ within the heart. The followers of Elias Hicks emphasized the individual's direct contact with God. The evangelicals, like others before them, fretted that such personal revelation undercut church authority.

Using diaries, correspondence, published and unpublished narratives, tracts, newspapers, and Quaker meeting records, H. Larry Ingle describes the confrontations of the decade or so prior to the schism, the process of separation in 1827, and its spread from Philadelphia to New York, Baltimore, and Ohio yearly meetings during the following year. Although he shows clear theological differences between the two groups, Ingle argues that the issue of authority was pivotal in causing the split. The evangelicals controlled Philadelphia Yearly Meeting despite their numerical minority. Well-to-do and worldly Philadelphians, they held the most influential posts within the yearly meeting. The Hicksite leaders gained their support mostly from rural districts, less affluent sections of Philadelphia, and the decidedly smaller city of Wilmington, Delaware. These self-styled reformers attempted to wrest control from the evangelicals in order to lead the society back to what they considered the true Quaker path.

Without question, Ingle has contributed to our understanding of the Hicksite separation. Using previously untapped sources, he presents what is at times a fascinating, detailed account of two sides marching to disaster. Ingle maintains a balanced view, but his emphasis on the question of authority rather than (or more correctly, in addition to) the theological issues leads him to disparage both sides as power-hungry and shortsighted. Many questions still need investigation. We need more rigorous prosopographical analysis of the Friends who took sides. The role of female Friends in the division remains murky. The events of 1827–28 should be examined with greater attention to the continuing, long-term antagonism in the Society of Friends (and the larger society) between individual expression and corporate authority, the dynamic central to the Hicksite separation. And, to find out whether the schism was truly a reformation, not simply a separation, it is necessary to carry the analysis forward from 1828. Ingle's book provides a firm foundation for such further study.

JEAN R. SODERLUND
Swarthmore College

HOWARD JONES. *Mutiny on the* Amistad: *The Saga of a Slave Revolt and Its Impact on American Abolition, Law, and Diplomacy*. New York: Oxford University Press. 1987. Pp. ix, 271. $22.95.

As an example of resistance, there are few instances in the nineteenth century to match the daring and determination of the Africans who revolted and seized the Cuban steamer *Amistad*, in June 1839. The fifty-three Africans, including four young children, had been purchased by José Ruiz and Pedro Montes in Havana and were being taken to slavery in Guanaja, when they struck for their freedom. Of the whites on board only Ruiz and Montes were spared. The Africans, led by Cinque, calculated that the two were needed to get the ship back to Africa. But the Cubans foiled their plans by sailing east during the day and north at night. Two months later, with provisions and water exhausted and ten Africans dead, the ship anchored off Long Island. While a party was on shore the *Amistad* was seized by an American revenue cutter and the Africans taken as prisoners to Connecticut. For the next two years abolitionists fought to free the Africans in what became a cause célèbre for the movement.

Historians of the antebellum period and the abolitionist movements are familiar with the details of the revolt and the general contours of the legal battle. In this study Howard Jones unravels the complicated, almost byzantine, legal tangle of the case, which made its way to the Supreme Court, and discusses its ramifications for abolitionism, law, and diplomacy. His analysis of the revolt's impact on relations between Spain and America is informative, as are his revelations concerning the extent of Martin Van Buren's efforts to subvert the judicial proceedings for political ends. Years later, the House Foreign Relations Committee would attempt a similar ploy in an effort to mollify Spain, calculating that indemnities for the *Amistad* would facilitate American acquisition of Cuba.

Jones is good—even lyrical at times—when discussing the impact of the case on law and diplomacy. His attention to its impact on abolitionism, however,

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is less satisfactory. Two things cause me concern. One, he asserts repeatedly that the case had a profound impact on the abolitionist movement but does not delineate its nature. One question will suffice. If Justice Joseph Story's brief, as Jones rightly observes, established the right to slaves as property and in so doing undermined the abolitionist arguments for the primacy of natural rights, what impact did this have on the movement, and how did abolitionists deal with the issue? One can raise a number of similar, broadly conceived questions that I think are pertinent. But one thing troubles me even more: what became of the black involvement in this incident? Logic would suggest, and facts show, that, in a decade when blacks were coming of age politically, the case became a rallying point for black communities in the Northeast, especially those in Hartford and New Haven. For example, while the New York Amistad Committee was responsible for organizing the defense, blacks in Hartford, led by J. W. C. Pennington, were busy making plans for the day when the captives would be free. Later, they formed the Union Missionary Society, the first avowedly abolitionist missionary society in the country, with the express purpose of using the Mendians as the nucleus of a missionary station in Africa. Two years later, the society merged with other missionary societies to form the American Missionary Association. That role should have been included. Had Jones consulted the *Colored American*, he would have found ample evidence of black contributions to this cause.

RICHARD BLACKETT
Indiana University

CLARENCE L. MOHR. *On the Threshold of Freedom: Masters and Slaves in Civil War Georgia*. Athens: University of Georgia Press. 1986. Pp. xxi, 397. $35.00.

Scholars of race relations have tended to scrutinize the American South before the Civil War and during Reconstruction, leaving a fascinating time gap during the war itself. Received wisdom held that slavery continued much as it had until the moment Union armies arrived on the scene and liberated slaves, the turning point between antebellum and postbellum race relations. Clarence L. Mohr successfully challenges this traditional interpretaion of race relations by using the crucible of the Civil War to study southern society in Georgia.

Deftly combining social and intellectual, narrative and quantitative history, Mohr crafts a sensitive and imaginative story of a society accepting change to win a war against change. However obliquely, Georgians faced the contradictions in a system that viewed blacks as both people and property, and, by Appomattox, blacks and whites had accommodated themselves to increased black freedom. Mohr argues this accommodation linked antebellum slavery and Reconstruction.

Slavery constituted a "dynamic nexus," a living relationship between exploiter and exploited. Beginning with panic in the aftermath of John Brown's raid, Mohr documents white paranoia when violence replaced a relatively peaceful paternalism as the mode of race relations. As war approached, stricter enforcement of antiquated regulations circumscribed slave life and threatened free blacks with enslavement. Mohr develops a complex and compelling interpretation for black submission to terror and suspicion. When the war actually began, however, Afro-Americans became conscious of white weaknesses and their own opportunities for freedom. Southern race relations continued to change over the course of the war, and slavery became increasingly more difficult to enforce. Mohr covers the varied experiences of blacks as a result of wartime urbanization, refugeeism, absence of white men, and the demands of new industrial and military assignments. And he makes a major contribution to the history of slave runaways by detailing the complexity of a fugitive's decision. He estimates that two thousand blacks joined Union lines in the first year of the war and discovers a change from single runaways to families, indicating both the importance of kinship and the decrease of white control.

Partly because of slave runaways, whites fled from the war zones. Mohr believes that by 1863 a majority of Georgians experienced some aspect of the refugee movement. Relocated, slaves bore the brunt of hardships imposed by war. Whereas some masters diversified plantations and forced slaves to learn new skills or accept additional tasks, others rented their slaves for military and industrial work.

Refugeeing (a word common at that time) proved to many slaves that whites adhered to paternalism only when convenient. Whites also abandoned paternalistic responsibilities by reducing allowances of food and clothing to slaves. Masters further revealed their vulnerability when they failed to protect slaves from Confederate impressment.

Blacks in urban Georgia increased their freedom during the Civil War. At the front, in cities, and in factories, provincial slaves mingled with the more sophisticated. Slaves gauged their labor's value to the war effort and increased their bargaining power. From sketchy but consistent evidence, Mohr posits that blacks in rural Georgia also experienced more freedom. As the battlefield took white men, vacancies presented unprecedented opportunities for blacks to control daily activities. Under these circumstances some slaves "cast off the psychological chains of bondage" (p. 223).

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In a fascinating chapter Mohr argues that the Civil War ended the intellectual barriers against criticism of the peculiar institution. Building on the work of David Brion Davis, Mohr believes reform stemmed from both the earnest desire to fulfill the conservative ideal of slavery as a paternalist system and the liberal ideal of inevitable human progress. Evangelical Christians linked the two sources of reform; although they never asserted black equality except as recipients of God's grace, southern evangelicals moved closer to the abolitionists' position that, as humans, blacks deserved equal treatment. We should see further study of these ameliorative gradualists and their role during Reconstruction.

Through his regional study Mohr conveys the complexity of Afro-American society and differences in antebellum free blacks and slaves. Through careful reasoning the author suggests important interpretations of Civil War society and poses alternative explanations of the evidence. This well-written and discerning monograph is a model study on an important subject.

ORVILLE VERNON BURTON
University of Illinois

RUSSELL DUNCAN. *Freedom's Shore: Tunis Campbell and the Georgia Freedmen*. Athens: University of Georgia Press. 1986. Pp. xiv, 175. $20.00.

Reconstruction Georgia produced three premier black leaders: Henry M. Turner, A. A. Bradley, and Tunis G. Campbell. Sr. Russell Duncan's book deals with the most successful of these three in establishing an impressive and fairly long-lasting local black political base. Eschewing the standard political history, Duncan "presents an explanation of Tunis G. Campbell's tactics to change labor and social relationships in Georgia from ones of inequality to ones of equality" (p. xi). Organized chronologically, three basic chapters trace the New Jersey–born minister from his appointment as an official of the Freedman's Bureau on the Georgia Sea Islands to his establishment of an impressive political machine in McIntosh County, where he served as both a local magistrate and state senator.

Duncan makes a convincing case that Campbell's success emanated not so much from his charisma as from his mirroring of the hopes of his constituents. This well-written book documents how Campbell used his power arbitrarily but totally to the benefit of his constituents. Despite unbelievable white persecution, which the author documents in rich detail, the aging black minister survived long after Georgia was "redeemed." Thus, Campbell emerges from these pages as a "freedom fighter" (p. xi).

Given his perspective, the author sometimes bends over backward to defend the preacher-politician. In refuting charges that Campbell pocketed money destined for freed slaves, or at least illegally paid them in liquor, Duncan finds it "highly unlikely that a minister of the African Methodist Episcopal church and a temperance lecturer for more than thirty years would have" so acted (pp. 33–34). Campbell was simply high-handed. His own stepson said "to defy him imperiled one's life and property" (p. 87). When Bradley attempted to enter Campbell's political turf, the minister's supporters physically ran him out of the county. Duncan concedes that Campbell was "a powerful politician—similar to Huey Long or Adam Clayton Powell" (p. 69). Campbell found himself in a dilemma. To help his constituents, he sometimes had to act arbitrarily, but in so doing he confirmed white fears of black domination. Duncan has helped us appreciate the frustration Campbell must have felt, as well as the personal persecution he experienced.

EDMUND L. DRAGO
College of Charleston

LAURENCE SHORE. *Southern Capitalists: The Ideological Leadership of an Elite, 1832–1885*. (Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1986. Pp. xii, 282. $25.00.

On one level Laurence Shore's book is about the ideas advanced by men who belonged to the elite in the nineteenth-century South in an effort to hold their positions amid great upheaval. Faced first with the challenge of defending an institution out of step with the prevailing political economy of Adam Smith and later with the equally daunting task of reestablishing their credibility after defeat and reordering southern attitudes toward labor, these leaders shaped their rhetoric in pragmatic attempts to maintain white unity under their leadership. On another level the book raises again the question of whether the Civil War was the great watershed in southern history and suggests that continuity, not contrast, is the theme of the nineteenth-century South. On both levels it stimulates and challenges, even where it does not persuade.

With the triumph of classical economics in Western thought in the 1830s, southern leaders recognized the need to shift the defense of slavery to economic ground. Shore asserts that Thomas Dew led the way with his essay on the Virginia legislative debate on slavery in 1832, which was pivotal in that it attempted to show that "our adversaries are not justified in their conclusions, even if we admit the truth of their premises." Dew asserted the economic viability of slavery for the foreseeable future and

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added that the nature of the African and the climate of the southern states made it necessary.

Although Shore finds an ample cast of "slaveholding capitalists," a label he feels "captures how proslavery ideologists thought of themselves" (p. 200), he shows that the tensions inherent in this term split the southern mind by the 1850s, as seen in the views of George Fitzhugh and Hinton Rowan Helper. While neither man had a large following in the South, the disunity they illustrated would undermine the attempt at reconciliation and doom the peculiar institution. Unable either to expand slave ownership or to idealize work and sacrifice amid racial slavery, the Old South died in a "rich man's war" fought by poor men insufficiently wedded to its ideology.

Turning to the postwar period, Shore adds a new dimension to the debate on the nature of southern leadership. Until recently the prevailing view has been that advanced by C. Vann Woodward in *Origins of the New South* (1951), that following the war a new elite emerged, not tied to the values of the past and therefore willing to embrace those of the victorious North. Lately a new school has challenged Woodward, pointing to the "Prussian road" followed by a postwar reactionary elite. Shore suggests that the "New South" leaders were neither reactionaries nor new men. They were the same men who had led the Old South, and the reconciliation with capitalism that they called for and hoped to lead was for them easy since they had been capitalists all along. Pragmatic conservatives such as Alexander Stephens, L. Q. C. Lamar, and Zebulon Vance sought the preservation of white unity through wider landownership and economic diversification.

Shore's research is impressive, and he makes imaginative use of concepts borrowed from Kenneth Burke and from J. G. A. Pocock, who suggests that "a power structure may survive by successfully transforming its idiom" (p. 111). Although his thesis (which is best expressed on page 13) required him to work in a broad time frame, he has neglected spokesmen who saw the South as blessedly anticapitalist, and his evidence sometimes fails to prove his point (see table, p. 238). He nonetheless makes an important contribution to the issues of uniqueness and change over time in southern history.

JAMES O. FARMER, JR.
University of South Carolina, Lancaster

EDWARD J. DAVIES II. *The Anthracite Aristocracy: Leadership and Social Change in the Hard Coal Regions of Northeastern Pennsylvania, 1800–1930*. DeKalb: Northern Illinois University Press. 1985. Pp. 277. $27.00.

Edward J. Davies II offers here a painstakingly careful analysis of the social and economic activities of business leaders in two coal towns between about 1850 and 1900. A comparison of the two cases demonstrates, he argues, that local social patterns play as important a role as the forces of economic geography or corporate strategy in determining a town's fate. Although his evidence does not fully support his larger argument, his book significantly enriches the literature on urban elites and the economic development of towns, cities, and regions.

Nineteenth-century Wilkes-Barre, Pennsylvania, developed an "upper class," Davies shows, bound by intermarriage, residence in favored neighborhoods, membership in select Episcopalian and Presbyterian churches and in exclusive social clubs, attendance at preferred private schools and colleges. Born into families with ties to elites in Philadelphia, Boston, and New York, members of this group developed further social and economic connections in those cities. They also monopolized important local political offices. Using these resources, they promoted the Pennsylvania Main Line Canal and then, in the 1850s and 1860s, a network of rail lines to connect nearby hard coal fields to the eastern markets. Their coal operations and related enterprises prospered, and by the late 1860s their wealth and their social institutions were attracting successful entrepreneurs from neighboring towns, consolidating Wilkes-Barre's dominant place in its region.

Pottsville, fifty miles to the south, did not produce a comparable upper class despite a similar (but somewhat earlier) boom in the anthracite fields in its immediate vicinity. In contrast to their Wilkes-Barre counterparts, the men who prospered during Pottsville's boom "shared little by way of class origins, lineage, or ethnocultural traits" (p. 129). Although it was similar in size (though apparently not in wealth) to Wilkes-Barre in 1870, Pottsville had an economic elite only in a statistical sense: its wealthiest men never established strong incorporated business firms or created effectively inclusive social organizations; they never dominated local political offices or pursued a common policy for their town's economic development. Philadelphia corporations took complete control of Pottsville's coal and iron businesses during the period of consolidation in those industries that followed the depression of the 1870s. Several Pottsville entrepreneurs failed; many plants and collieries were closed; the town never recovered.

Davies shows a splendid mastery of the sources and provides a convincing contrast between the business leaders of the two towns. He does not show, however, that Wilkes-Barre sustained its prosperity—within the bounds allowed by an expanding Scranton—because its local upper class was able to marshal its capital, pull together, and take the long

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view; nor does he show that Pottsville declined because its elite failed to do these things. Would not straightforward economic forces, which he does not fully consider, have produced the same results in any case? As Pottsville's coal played out and as railroad and telegraph communication improved, it became more efficient to shut most small collieries and mills and to manage those that remained from Philadelphia. Wilkes-Barre, conversely, served New York as well as Philadelphia and provided goods and services (and jobs for the wives and children of miners) for an anthracite industry that earned profits into the 1920s. Davies's welcome contribution to urban economic and social history tells us less than he hoped about the economic consequences of values and social institutions.

DAVID C. HAMMACK
Case Western Reserve University

STUART B. KAUFMAN *et al.*, editors. *The Samuel Gompers Papers*. Volume 1, *The Making of a Union Leader, 1850–86*. Urbana: University of Illinois Press. 1986. Pp. xxxvi, 529. $39.95.

This book, the initial volume of a projected multivolume printed edition, is the latest product of the Samuel Gompers papers project begun under the direction of Stuart B. Kaufman in 1973. Over the years the project staff has collected in excess of one-half million documents, three hundred thousand of which appeared in a 1979 microfilm edition entitled, "American Federation of Labor Records: The Samuel Gompers Era." A second microfilm edition, "The American Federation of Labor and the Unions," is planned. This is the first such project to be exclusively dedicated to a working-class figure.

As the earlier microfilm edition and now the first of the printed volumes make abundantly clear, the Gompers collection and Kaufman's skillful management of the editorial project are both highly praiseworthy. The author of an earlier, somewhat controversial study of the social and intellectual sources of Gompers's economic and trade union philosophy, Kaufman did his apprenticeship on Louis Harlan's acclaimed Booker T. Washington papers project, and, indeed, there is striking evidence of the mentor's influence in this project.

Having said all of that, I must report that this first volume of the Gompers papers is somewhat disappointing. One actually gets a better feel for the man—what he thought, how he operated, why he was so successful—from reading a volume or two of the Gompers Letterbooks in the Library of Congress. The problem is that most of the early manuscripts involving Gompers and his activities with the Cigarmakers International Union, the Federation of Organized Trades and Labor Unions (FOTLU), and the early American Federation of Labor (AFL)—manuscripts that Gompers assiduously collected and preserved—were destroyed during the year (1895) that John McBride succeeded Gompers in the AFL presidency and union headquarters were moved from New York to Indianapolis. As a consequence, the documents in this volume, most of which were drawn from such printed sources as newspapers and union journals, more accurately reflect the public than the private man. As it is, this volume is much more a documentary history of the early, formative years of the modern American labor movement than a collection of Gompers manuscripts as such.

Given that caveat, what the editors do, they do extremely well. The first twenty pages contain documents covering Gompers's London childhood and his early years in New York City. Following are sections concerning Gompers's introduction to socialist thought, his experiences in the cigarmaking industry and association with trade unionism, the organization of the AFL, its predecessor, the FOTLU, and its most bitter rival, the Knights of Labor, and two of the more volatile movements of the day, the campaign for the eight-hour day and Henry George's single tax.

Each of the fourteen sections is preceded by a five hundred to one thousand word introduction that establishes the historical context and identifies the historical significance of the documents that follow. The documents are reproduced with as little editorial intervention as possible and are well but not excessively annotated.

Not too surprisingly, the documents, especially those covering Gompers's early years, tend to support the argument advanced in Kaufman's monograph, *Samuel Gompers and the Origins of the American Federation of Labor, 1848–1896* (1973), concerning resilient Marxist influence in Gompers's evolving social and economic thought. Conversely, those documents related to conflict between the AFL and the Knights of Labor and to socialist challenges to AFL policies and leadership illustrate a conservative strain in Gompers's evolving trade union philosophy that would become increasingly dominant in his later years.

GARY M FINK
Georgia State University

RUTH BORDIN. *Frances Willard: A Biography*. Chapel Hill: University of North Carolina Press. 1986. Pp. xv, 294. $25.00.

Frances Willard (1838–98) was one of the most famous, respected, and influential women in nineteenth-century America, but no biography of her has been published in the last forty years. Having

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established herself as an able chronicler of the Women's Christian Temperance Union (WCTU) in *Women and Temperance: The Quest for Power and Liberty, 1873–1900* (1981), Ruth Bordin has now written a sound portrait of temperance's primary exponent. Using diaries and scrapbooks that had been lost since the turn of the century, Bordin illuminates both the private and the public life of a skilled leader with a broad social vision. Willard did not appear to challenge society's accepted ideals about women's traditional virtues, but she pressed them into service, awakening hundreds of thousands of female temperance advocates to realize their own power by supporting a "Do Everything" policy for "Home Protection."

Willard initiated impressive programs, from cooperative kitchens to free kindergartens, social purity, and female suffrage, which her followers developed, ultimately making a great impact as reformers. Surprisingly, Bordin manages to avoid rehashing material she treated in her earlier book. Instead, she argues that Willard, the person, can be disentangled from temperance history to demonstrate her strong commitment to a broad range of social and political causes, including Christian Socialism, transatlantic feminism, and the labor movement. Although Bordin is quick to praise Willard's eloquence as a public speaker, her astuteness as a politician, and her success in carrying out an ambitious social program, she does not ignore her shortcomings. She scrutinizes Willard's hypocrisy on the issue of lynching, her selfishness in her personal relationships with Kate Jackson, Anna Gordon, and Lady Henry Somerset, and her differences with the Methodist clergy, Elizabeth Cady Stanton, and the WCTU factions opposed to her support for women's suffrage and for the Prohibition party. Bordin succeeds, too, at synthesizing, coherently but succinctly, the backgrounds of numerous mass movements that Willard supported. Finally, Bordin's survey does not topple the assumptions about Frances Willard with which women's historians have worked in recent decades, but it chronicles them fully, with great care and persuasiveness.

KAREN J. BLAIR
Central Washington University

WILLIAM R. MAJORS. *Change and Continuity: Tennessee Politics since the Civil War*. Macon, Ga.: Mercer University Press. 1986. Pp. x, 127. $9.95.

Students of Tennessee history suffered a sad loss with the untimely death of William R. Majors in April 1986. He was a thoughtful analyst of Tennessee politics and the author of important works on Gordon Browning and Edward Ward Carmack. His third book, *Change and Continuity: Tennessee Politics since the Civil War*, reflects his years of careful research in the field and expresses some strong ideas about the political heritage of the Volunteer State. Although conceding that Tennessee has experienced enormous changes over the last 125 years, Majors aligns himself with those who stress continuity rather than disruption in southern history. In Tennessee, he claims, "a basic cultural pattern . . . or more specifically a Bourbon conservatism has persisted relatively unaltered" (p. ix). Echoing the sentiments of George Tindall and others, he finds in Tennessee a conservative "community of interests," which "when faced with change or demands for reform adjusted, accommodated to reform, and reconciled innovation with tradition" (p. 110). In the process Majors also builds on an interesting paper he presented at the 1981 meeting of the Southern Historical Association in which he argued that V. O. Key had misinterpreted some important elements of the state's politics in his chapter on Tennessee in *Southern Politics in State and Nation* (1949). The present book challenges Key's emphasis on the "Civil War and Mr. Crump," contending that the state's basic political patterns were established before the Civil War and that the structure of political debate owes more to the Bourbon period than to the wartime era.

With this book, Majors has done a real service to the study of Tennessee history in the twentieth century. A smoothly written, interpretive survey of Tennessee's past, the book addresses a number of important issues in state and regional historiography. It is especially well suited for use in Tennessee history classes at the high school or college level. Perhaps the most disappointing part of the book is its bibliographical essay. Majors has been content to summarize the literature and has thereby missed an opportunity to provide a sharper and more biting critique of the available scholarship. Nevertheless, this book offers students a fine introduction to the state's past and provides experts a challenging framework for analysis.

DAVID D. LEE
Western Kentucky University

WILLIAM A. LINK. *A Hard Country and a Lonely Place: Schooling, Society, and Reform in Rural Virginia, 1870–1920*. (Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1986. Pp. xii, 275. $26.50.

William A. Link explains in his preface that the "main purpose in this book is to understand the rural past through a study of its schools" (p. ix). He divides the study into two sections. Part 1 (chaps. 1–3, "Localism and the Rural School") considers the plight of education during the post-Reconstruction

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era and the historical forces that molded its development.

Part 2 (chaps. 4–8, "The Modern School") succinctly treats the origins of the Southern Educational movement, embracing the Capon, West Virginia, conferences, the Ogden movement, the Winston-Salem Conference, and the Southern Education Board (SEB). The SEB appointed two agents, Henry St. George Tucker and Robert Frazer, who were the mainspring of the educational crusade in Virginia. They converted to their cause the press, civic groups, local elites, and state leaders, notably Governor Andrew Jackson Montague and his successor, Claude Augustus Swanson. Important allies of the educational campaign in the state were the Co-Operative Education Association and the Richmond Education Association. Women participated, and Link includes interesting vignettes, with large pictures, on Lila Meade Valentine, Mary Cooke Branch Mumford, and Virginia Estelle Randolph of the Jeanes Fund.

In 1905 the reformers launched the "May Campaign"—later described as a "combination of crusade and a glorified picnic." In the background were leagues of women, who "were like fans at a baseball game; they organized the crowds, cheered the speakers, and served as the backbone of the leagues" (p. 114). A discussion of the results and success of the May Campaign is disappointing.

This study has its shortcomings. Although Link includes a map showing the state's geographic regions, he does not designate the Black Belt counties—the contiguous counties in which blacks constituted at least 40 percent of the population. Indeed, the Black Belts are the foundation of V. O. Key's thesis presented in *Southern Politics* (1949). Here, a white minority—merchants, planters, and lower-class whites—manifested the strongest feelings about the maintenance of white supremacy and opposition toward public education, especially for blacks. Moreover, as Louis R. Harlan in *Separate and Unequal* (1958) demonstrates, "both white and black counties" agitated for a racial division of school taxes (all taxes paid by whites to be used exclusively for white schools). But Link never comes to grips with the problem. And despite his use of extensive primary sources and standard and newer secondary works, he does not take advantage of similar studies. Neglected especially, for example, were Horace Mann Bond's *Negro Education in Alabama: A Study in Cotton and Steel* (1939) and Leon Prather's *Resurgent Politics and Educational Progressivism in the New South: North Carolina, 1890–1913* (1979). Furthermore, it is hard to explain the omission of a bibliography or an essay on sources. Finally, Link does not establish a broader perspective for the Virginia public school movement by identifying it with the legacy of southern Progressivism. Indeed, the laudable expansion of public school opportunities for the regional masses was the most discernible manifestation of southern Progressivism. "No aspect of social reform in the South during the progressive era," Dewey W. Grantham wrote, "touched the lives of more people than the great educational awakening soon after the turn of the century" (*Southern Progressivism: The Reconciliation of Progress and Tradition* [1983]).

Notwithstanding these negative comments, this book certainly is not a paradigm of traditional educational histories. Meticulously documented and well-written in a classical academic genre, it contributes a valuable addition to educational historiography.

H. LEON PRATHER, SR.
Tennessee State University

THOMAS E. SHERIDAN. *Los Tucsonenses: The Mexican Community in Tucson, 1854–1941*. Tucson: University of Arizona Press. 1986. Pp. xiv, 327. $22.50.

Community histories represent the foundation in the development of Chicano historiography during the 1970s and 1980s. Studies by Albert Camarillo, Richard Griswold del Castillo, Ricardo Romo, and Francisco Balderrama, for example, have revised the history of Los Angeles and Santa Barbara by their attention to the Mexican-American presence. Mario T. Garcia and Oscar Martinez have examined the role of Mexicans in the Texas border community of El Paso, and Arnoldo De Leon in his broad social history has done the same for the rest of Texas. Several doctoral dissertations—some to be soon published—have likewise concentrated on urban Mexican communities in the Southwest and Midwest. A gap in the literature, however, has been community studies in Arizona and to a lesser extent New Mexico. Fortunately, Thomas E. Sheridan's book now significantly helps fill this gap.

This is a rich and wide-ranging history of Mexicans in Tucson from the post–Mexican War era to World War II. No other community history on Mexicans in the United States has been as ambitious as Sheridan's. Meticulous in his research of key primary sources such as census material and Spanish-language newspapers, Sheridan breaks no new ground in the conceptualization of Chicano history. That conceptualization dramatically revises the mythic frontier thesis first propounded by Frederick Jackson Turner and Walter Prescott Webb that incorrectly stresses the westward expansion of democracy and freedom. In its place Chicano historiography, along with other ethnic-oriented literature, offers a more balanced perspective emphasizing racial, ethnic, and class conflict. Sheridan's study augments this revisionist focus.

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Sheridan records the decline of Mexican rule and influence in Tucson in the years following the Mexican War and the ultimate relegation by the twentieth century of most Mexicans to second-class citizens, serving essentially as pools of cheap and surplus labor. Yet his study is more than a history of victimization. Like its counterparts in Chicano historiography, this book calls attention to the active role of Mexicans in responding to these fundamental changes. Indeed, the richness of Sheridan's study lies in the bringing to light of the vibrant social, cultural, and business life of the increasingly segregated Mexican *barrios* of Tucson. Here, Sheridan's discussion of a nascent Mexican commercial class in the post–Mexican War years, the organization of mutual benefit societies such as the Alianza Hispano-Americana, and the interplay of middle-class Mexican cultural life with working-class popular culture is of particular interest. By countering a history of increased exploitation of the Mexican with a history of community building, Sheridan has produced a holistic assessment of what it meant to be a Mexican in Tucson and, indeed, throughout most of the Southwest.

Sheridan's study, however, does falter in conveying well the changes in historical experiences within the Mexican community. Sheridan notes changes in status and consciousness but does not formulate these changes into a more clearly defined periodization. The Mexican community in Tucson, as elsewhere, experienced in a broad generational sense at least three significant periods from the Mexican War to World War II. These can be conceptualized as the era of the conquered generation in the post–Mexican War years, when Mexicans reacted to the central experience of the U.S. conquest; the era of the immigrant generation during the first three decades of the twentieth century, when immigration formed the central experience; and the era of the Mexican-American generation by World War II, when a new generation of Mexicans forged a twin struggle for first-class citizenship and a new identity as Americans of Mexican descent.

Still, Sheridan's study represents a valuable new addition to Chicano and southwestern community history.

MARIO T. GARCIA
University of California, Santa Barbara

JOHN FAHEY. *The Inland Empire: Unfolding Years, 1879–1929*. Seattle: University of Washington Press. 1986. Pp. xii, 298. $19.95.

The Inland Empire is a generally recognized region (or subregion) that has Spokane, Washington, as its focal point. At its furthest extent the region includes the area between the Cascade and Rocky mountains and extends from the southern portions of British Columbia and Alberta into eastern Oregon. And, as John Fahey notes, the Inland Empire is also "a state of mind as well as a place" and expands and contracts (p. xi). For his purposes, Fahey confines the region to "the counties of Washington east of the Cascade range and those of northern Idaho linked by mining and lumbering to Spokane." Within this geographic framework, "this book attempts to provide a cohesive narrative of the major social and economic themes in the emergence of the Inland Empire as an integrated region" (p. xi).

The book's major objectives are deeply flawed. There is no logical explanation for its time frame; major trends and policies that began in the 1920s are left hanging by the arbitrary cutoff in 1929, a date that has little meaning for an agrarian area that was in a depression for most of the 1920s. The book primarily deals with economic topics including railroads, grain, apples, banking, irrigation, mining, and lumber. Many of the chapters are commendable, but taken as a whole they do not provide a cohesive narrative of a region. Further, the geographic framework is both too limiting and too inclusive. Two of the more informative chapters treat the development of orchards and irrigation in the early years of the twentieth century. But these activities primarily took place in the tier of counties (Okanogan, Chelan, Kittitas, and Yakima) on the eastern slope of the Cascade range. This area, looking to Seattle, not Spokane, as the metropolis, has not identified with the Inland Empire. In fact, the two themes that Fahey emphasizes—orchards and irrigation—are among the major reasons these counties have a distinct identity. On the other hand, there are numerous social, political, and economic ties (in addition to mining and lumber) that bind much of Idaho, Montana, and the two westernmost Canadian provinces to the Inland Empire. Any edition of the region's major newspaper, the *Spokesman-Review*, continues to give evidence of these connections.

If the book's chapters are treated as individual essays, the result is better. Donald Meinig and Fahey have written more extensively elsewhere on some of the topics, such as agriculture, mining, and railroads, but the chapters on other topics—orchards, banks, irrigation—are as good as or better than anything else available. A chapter particularly worthy of note is titled "Machines" and is an account of the dramatic impact that automobiles, telephones, and electricity had on the lives of rural dwellers.

Fahey's book adds up to less than the sum of its often excellent parts. This elusive region still awaits the historian astute enough to capture it.

KENT D. RICHARDS
Central Washington University

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SUSAN PORTER BENSON. *Counter Cultures: Saleswomen, Managers, and Customers in American Department Stores, 1890–1940.* (The Working Class in American History.) Urbana: University of Illinois Press. 1986. Pp. xvi, 322. $27.50.

Susan Porter Benson's study of the rise of American department stores describes the intersection of women's history with the new labor history and places them within the context of business history. Benson artfully combines these several themes in a complex mosaic of class, gender, and work culture within an emerging consumer society, with the department store as the modern "palace of consumption." She analyzes the rise of sales work as an occupation that provided room for a certain measure of creativity, originality, and worker control. The skills that the profession required reinforced the concept of a women's culture, that is, an emphasis on interpersonal skills, along with a knowledge of fashion and quality in consumer merchandise.

As Benson points out, by the late nineteenth century, selling had become well established as a women's occupation. The number of saleswomen jumped from under eight thousand to over fifty-eight thousand between 1880 and 1890, and by 1940 women constituted 41 percent of all sales workers. There were several explanations for the preference for female sales personnel in department stores, but perhaps the most obvious were the large supply and low cost of women's labor. Sales work developed similarly to clerical work in that women were hired, paid, and treated as incumbents of dead-end jobs.

The status of saleswomen was a function of the rationalization of department stores and the managerial quest for efficiency and profits through control of the work force and education of the consumer. Employers wanted to change the class-based characteristics of their sales force and co-opt its gender-based characteristics to appeal to the middle-class female consumer. But, as Benson observes, saleswomen had their own ideas about their work. The concept of work culture ("the ideology and practice with which workers stake out a relatively autonomous sphere of action on the job" [p. 228]) is used to analyze the interaction between managers, saleswomen, and customers. Benson argues that the work culture of department store saleswomen reflected a consciousness of themselves as workers, as women, and as consumers. In fact, they were "an extreme case of the dilemma of all workers under consumer capitalism—driven by the social relations of the workplace to see themselves as members of the working class, cajoled by the rewards of mass consumption to see themselves as middle-class" (p. 271).

Benson goes beyond the work of labor historians Harry Braverman, Richard Edwards, and Dan Clawson, which is primarily concerned with the male-dominated shop floor, to reinforce a feminist perspective of history: women's historical experience was often different from that of men, whether it occurred in the family or in the marketplace. For, whereas the new labor history notes the movement toward scientific management and the degrading of work, Benson argues that department store managers went against the grain of labor-management trends of the twentieth century. They were attempting to foster skill—in particular, skill in interpersonal relations—and to harness that skill in a way that would attract customers, improve sales, and increase profits. Thus, scientific management, as it took place on the factory floor, could not be replicated on the sales floor, and the saleswoman had a degree of latitude in dealing with the public that could not be controlled by her employer.

Benson bridges women's history and labor history with her analysis of class, culture, and gender. Her insightful interpretation of a twentieth-century women's culture based on consumerism and the affluence of the department store, as well as the interpersonal relationship between women as consumers and as workers, offers a new perspective in American cultural and economic history. Her study includes useful appendixes, which provide a statistical account of the rise and status of female workers in the retailing business, and she makes good use of the retail trade literature to analyze the goals of managers and owners who became involved in the transformation of the industry along the lines of modern management. This is a valuable contribution to the field of labor history and to the recent discussion surrounding the concept of a distinct women's culture in American history.

WINIFRED D. WANDERSEE
Hartwick College

ALLAN KENT POWELL. *The Next Time We Strike: Labor in Utah's Coal Fields, 1900–1933.* Logan: Utah State University Press. 1985. Pp. xix, 272. $19.95.

The subject of coal mining has never been a particular favorite of historians, especially those who research the American West. Of the western coal districts, Utah has probably captured the least national attention, except for two tragic coal mining disasters. Even the unions found it difficult to reach the state's coal miners, let alone achieve success in organizing them.

Allan Kent Powell has finally given these forgotten miners their hour on Clio's stage. The present book is a concisely organized, beautifully written, and moving story that might well serve as a model

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for others who are considering similar efforts for their states. With a few more balanced monographs such as this one, western coal mining's historic role will be much better understood.

Powell's book opens with a general survey of the Utah coal industry that extends to the turn of the century; then it moves into the catastrophic Winter Quarters Mine disaster, near Scofield, on May 1, 1900. The plight of the miner and his dangerous working conditions come into sharp focus when one realizes that the exact number of men killed that day will never be known; it was estimated that at least two hundred died. Beginning from that grim day, the author traces the struggle to improve the lot of the miner in camp and in mine and the attempts to unionize. Success finally came, thanks to the National Industrial Recovery Act of 1933. After reading about this thirty-three-year struggle, the reader will readily understand why Utah's coal miners loyally voted for Franklin D. Roosevelt.

The book is not simply a local study of limited interest; Powell skillfully avoids falling into that trap. Against the background of a Mormon state with Mormon labor attitudes ("the average native of Utah is . . . the hardest biped to unionize in all America," complained one labor leader [p. 201]), Utah's coal mining history is developed within the framework of regional mining. Interesting comparisons are drawn with neighboring Colorado, which suffered through even more trying times, and with Wyoming, whose unions offered assistance. The underlying conflicts between immigrant and American-born miners and between Mormon and non-Mormon miners are carefully drawn and analyzed. Management proved adept at using the tensions to its advantage. Sensitive to all factions, Powell creates a professionally balanced presentation.

Powell works with three major themes: labor relations in Utah, the establishment of the union, and, finally, the role of the coal industry in hastening the end of one of America's last frontiers (p. xvii). He succeeds in interpreting these themes and in bringing to life the people and the times of a little understood, isolated world in central Utah.

DUANE A. SMITH
Fort Lewis College

DON M. COERVER and LINDA B. HALL. *Texas and the Mexican Revolution: A Study in State and National Border Policy, 1910–1920*. San Antonio, Tex.: Trinity University Press. 1984. Pp. 167. $15.95.

This monograph tells the story of two separate and failed searches for a successful American policy toward the Mexican revolution in the period from 1910 to 1920. The U.S. government sought, with little coherence, success, or luck, to formulate a foreign policy that protected broad national interests. The state government of Texas, principally during the administration of Governor Oscar B. Colquitt (1911–15), wanted a border policy that looked after more parochial state interests. Don M. Coerver and Linda B. Hall show that these two at times conflicting goals merely brought about much confusion and mutual recrimination between the two governments.

This state of affairs seemed inevitable. So many federal agencies dealt with immediate border problems and long-range diplomatic issues that it was often impossible to determine the scope and nature of federal policy. The watchful waiting of presidents William H. Taft and Woodrow Wilson only served to antagonize Governor Colquitt, who labeled Wilson's Mexican stance as "imbecilic." The governor, frustrated over issues of jurisdiction and with an eye toward serving his local constituency, wanted a tough, law-and-order reaction to bring about border peace and reopen international trade channels. Colquitt's overreaction to federal indecision and wishful thinking guaranteed a continuation of mutual frustration and irritation. Only at the end of the decade, with clearer, more realistic, visions of cooperation between Mexico City and Washington, could Governor William P. Hobby (1917–21) work with federal officials in cooling off intra-American tensions.

Basing their account on pertinent local and national archival materials, the authors tell their story well and explore the all-too-neglected regional component of U.S.-Mexican relations. The monograph would have benefited from a fuller discussion of several issues that were pivotal in understanding Texas's place in U.S.–Latin American relations. For example, what were the economic and political interests within the state that helped shape Colquitt's policies? Did popular opinion, apparently negative toward another Mexican tumult, play a decisive role?

Overall, this work is well worth reading. The historical dimensions of U.S. reactions to events and personalities south of the border offer much to think about regarding more contemporary issues. Not much has changed in this interplay of regional and national differences of interest.

LAWRENCE A. CARDOSO
University of Wyoming

DON L. HOFSOMMER. *The Southern Pacific, 1901–1985*. Foreword by RICHARD C. OVERTON. College Station: Texas A&M University Press. 1986. Pp. xviii, 373. $44.50.

A good history of the modern Southern Pacific (SP) is long overdue. Many lesser railroads of the twen-

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tieth century have received the attention of scholars, but SP has not, even though it is the dominant carrier of the west coast, a transcontinental route from California to New Orleans and St. Louis, and a participant in the historic overland route (the first transcontinental). Don L. Hofsommer's account describes the SP as it emerged at the end of the nineteenth century, a colossus of the West and the dominant force in California politics, under the control of E. H. Harriman. It continues with chapters on the tumultuous breakup of Harriman's empire, a stressful period of boom and depression, a shining hour in World War II when SP was the principal carrier to the ports of embarkation in the war against Japan, and a postwar period of changing traffic patterns, technological innovation, rising productivity, the end of steam and passenger operations, and railroad consolidation.

Hofsommer's account is primarily based on company and public documents, newspapers, and extensive interviews with the SP's present and past officers. It is documented with care and contains an excellent bibliography. Presented in a nine-by-eleven-inch format with lavish illustrations, maps, and reprints of significant memorabilia, it is an ideal melding of scholarship and the demands of a general and railroad-buff audience.

Southern Pacific has a rich history of opening the West and of running a vast and efficient network of rail, truck, and pipe lines that permitted the rise of whole industries and created jobs for millions. It was the originator of the Sprint communciations system and an early practitioner of an almost Japanese-style participatory management. On balance, it is an honorable history that needs no apologies.

But corporations are touchy about the less-than-uplifting chapters of their history, and the SP has some. Perhaps the largest landowner in California, the company holds rich timber and energy resources and has attained power and influence that some people have regarded as sinister. It was the villain of Frank Norris's novel *The Octopus*, which, though fiction, was thought to have factual foundation. The antitrust suits against SP in the 1910s and 1920s were not universally seen as frivolous exercises. Its exit from the passenger business in the 1960s may have been a sound business decision, but it was one of the nastiest of any American railroad. In the 1980s it had become a troubled company, described in the August 16, 1982, cover story of *Forbes* as "doomed?" Hofsommer appears to tell the story of these episodes the way the SP would like it told.

To write this history without access to company files would have been difficult, perhaps impossible. The author seems to have faced the classic dilemma of the business historian in that the price of access is to remain silent, or at least to put the best possible face on unpleasant episodes. Hofsommer is much too conscientious a historian to turn his book into an exercise in public relations, but his narrative needs development on the issues that would embarrass the SP.

This is an important, if not definitive, work by an excellent scholar and writer, a significant exploration in the history of business, technology, and the American West.

RICHARD SAUNDERS
Clemson University

ROGER L. GEIGER. *To Advance Knowledge: The Growth of American Research Universities, 1900–1940*. New York: Oxford University Press. 1986. Pp. x, 325. $27.50.

This book provides a fascinating account of how and why our major universities developed their most important attribute, that of being research institutions the equal of the best of their European counterparts. Although touching on other institutions, Roger L. Geiger concentrates his study on the sixteen charter and early members of the Association of American Universities. He maintains that the key element in the emergence of the sixteen as leaders among U.S. higher educational institutions was their success in acquiring operating funds. Much of the work, therefore, explores the successes and failures of the leaders at these institutions in securing these funds. Tuition charges, business support, and alumni and individual gifts are recognized, but the pivotal role in this regard played by large foundations in the accumulation of these resources is emphasized throughout the book. Subtly interwoven into this account of fund raising is the equally important one of how, with varying periods and degrees of success, the individual universities used these funds for research purposes.

Working within the above framework, Geiger presents an opening chapter describing higher education in the nineteenth century followed by two discussing the novel research programs put in place at the research universities during the period from 1900 to 1930. Included in the broad range of topics covered are the effect of selective student admission policies on research growth, the emergence of such entities as the Carnegie Institution of Washington and the National Research Council together with their relationship to the research universities, and the rewarding of faculty engaged in significant research with recognition and aid, particularly reduction in teaching loads.

Chapter 4 is devoted solely to the critical role of the large foundations in developing the research capabilities of the universities. Overall the author praises the contribution of the foundations, but he

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does not hesitate to point out failures. In discussing the operations of the Laura Spelman Rockefeller Memorial, for example, he provides an enlightening account of why one program funded by it at the University of Chicago was a success whereas a similar one at Yale was not.

Chapter 5 describes what is called "The Privately Funded University Research System," which had emerged by the time of World War II. Geiger traces business support of university research and provides explanations of how and why this support was important, in varying degrees, to the research universities. For example, the emergence of the California Institute of Technology as a great research institution is traced in large part to the interest and munificence of wealthy businessmen from southern California. Also provided are detailed profiles of the research structure in place by 1940 at six representative research universities: Harvard, Chicago, Princeton, Yale, Michigan, and California. A concluding chapter describes the negative impact of the depression years of the 1930s on the research universities and how governmental support became increasingly vital to them then and during World War II.

This book tends to put more emphasis on research developments in the natural sciences than in the social sciences and humanities. Also, there is a considerable amount of topical and chronological overlapping in the work. The nature of the subject, however, makes it difficult, perhaps impossible, to do a much better job in this regard. On balance, this book is the most understandable and best account of how and, as important, why our great modern research universities became what they are. This volume can be read with profit by all those interested in the history of U.S. higher education. It should be "must" reading for college and university administrators, particularly the heads of those institutions aspiring to join the ranks of the great research universities.

JOSEPH C. KIGER
University of Mississippi

DONALD T. CRITCHLOW, editor. *Socialism in the Heartland: The Midwestern Experience, 1900–1925*. Notre Dame, Ind.: University of Notre Dame Press. 1986. Pp. viii, 221. $21.95.

Since the end of World War II historians have worked hard to understand and explain the failure of socialist politics in the United States at the local, regional, and national level. Some of the histories and biographies have been distinguished contributions to our knowledge of the American Left and labor. But the question, why has there been no significant and continuing socialist movement in the United States, almost alone among Western industrial nations, has been asked so often and has brought such inconclusive answers that it now seems almost tedious. This volume of seven essays by seven authors on midwestern socialism proposes to answer that question on the basis of one section of the country, but Donald T. Critchlow's introductory essay acknowledges that such an analysis may shed more light on American society and culture than on the strengths and weaknesses of the American socialist movement vis-à-vis such movements elsewhere in the Western world.

It is difficult to draw a few clear and important conclusions from these essays. Six deal with local or urban party activities, the seventh with the state of Indiana. Only in Milwaukee, Sally Miller's assignment, was the political activity successful on a major scale over an extended period of time. The Minneapolis and Dayton experiences, described by David Paul Nord and John T. Walker, are not even remotely close in scale or significance. Of the three remaining sites, the United Mine Workers' District Number 12 in Illinois's Little Egypt, interpreted by John H. M. Laslett, seems the most interesting. The potentially related essays on Indiana socialism and the movement in Marion, Indiana, actually have no meaningful connection.

Aside from the Milwaukee experience, which is in a class by itself, the essays confirm what we already knew: that the Socialist party appealed to different social and economic constituencies from one community to another and even from time to time, that the character and quality of its leadership and of its electoral candidates varied greatly (as one should expect, despite their often holier-than-thou attitude), that sectarian and factional quarrels were endemic to the movement, that World War I and the Russian revolution and their accompanying stresses and tensions were destructive within the party and outside, although differing from place to place, and that other "popular" movements of the time, including the Ku Klux Klan, provided keen competition for Socialist party voteseekers. These studies confirm earlier conclusions that it was easier to win office, executive, administrative, and legislative, than to retain it or to accomplish much of a lasting nature, and the reasons are obvious: short terms, minority status or unstable and inadequate majorities, limitations on local tax resources, constraints of city charters and state laws and constitutions, the resilience of local institutions, and the resourcefulness of the socialists' opponents.

Perhaps it is unfair to expect too much from these seven short midwestern case studies when we are still unclear in our postmortem on the basis of larger local and national studies. I continue to believe that by the twentieth century the American political party system had learned how to deal with so-called

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third parties, among them the Socialist party, no matter their program or their people, and these essays tend to support this view. In Milwaukee, to illustrate the point, long after the party had ceased to be a political force, some of its ideas and principles and some of its members and former members remained a vital part of the local political scene. This is not political success as we normally measure it, but neither is it total failure.

There are serious deficiencies in editing and proofreading throughout this book. An example of a proofreading error is the discrepancy in the dates for the Marion study between the table of contents (1900–21) and the essay itself (1920–21). Errors in the index are common; more seriously, omissions are many. One expects better from a university press.

FREDERICK I. OLSON
University of Wisconsin, Milwaukee

RONALD LAWSON, editor. *The Tenant Movement in New York City, 1904–1980*. Assisted by MARK NAISON. New Brunswick, N.J.: Rutgers University Press. 1986. Pp. xiv, 289. Cloth $35.00, paper $15.00.

Housing has always been a neglected historical topic. The need for shelter touches everyone, but the circumstances surrounding this quest are so diverse, and so constantly in flux, that the issue is difficult to conceptualize over time. Traditional studies of discrimination in housing and the impact of slum conditions on immigrants and blacks have recently been supplemented by works that closely examine neighborhoods or small-scale environments, such as James Borchert's *Alley Life in Washington* (1980). At the same time, Robert Caro's *The Power Broker: Robert Moses and the Fall of New York* (1974) has alerted historians to the negative consequences on neighborhoods of highway construction and urban renewal since the 1930s.

This volume on tenant movements in New York City adds a valuable new perspective on the history of housing, and it may provide the missing ingredient that will make it possible for scholars to bring together diverse aspects of the housing issue to build a much needed historical synthesis. Five scholarly essays (as well as a pictorial essay by Ronald Lawson) trace the development of tenant activism from the sporadic, largely immigrant-based rent strikes of the early twentieth century, through the renewed militancy of the 1930s, to the highly organized lobbying activities of the post–World War II era, in which black tenant organizations have played an increasingly important role. The book clearly establishes that tenant organizations have exerted a far greater influence than historians have heretofore acknowledged. The history of rental housing, it seems, is no more a one-sided affair, with all power in the hands of the landlords, than the social history of industrial work is explainable solely through an analysis of management's goals and methods.

A number of interesting themes emerge from these essays. One is the importance of Jewish radicalism in shaping the early struggle for fair rents and decent housing. This was especially true of the immigrants who arrived before 1920, but second- and third-generation Jews continued to play an important role in the tenant movement as it reemerged in the 1930s and became more institutionalized in the 1950s and 1960s in such organizations as the Metropolitan Council on Housing. A second theme is the significance of women, in both rent strikes and tenant organizations. This aspect could have been developed more fully, perhaps, by exploring connections between tenant activism and other elements of women's lives and cultural values. Finally, to a greater extent than the editor is willing to admit in the introduction, this volume points up the weaknesses of Francis Fox Piven and Richard Cloward's *Poor People's Movements* (1979), which developed the romantic thesis that mass protest has helped social movements succeed, whereas organizations tended to limit the effectiveness of movements by undercutting militancy. In fact, it was the lack of organization that limited the early rent strikes, while the growth of stable tenant organizations beginning in the 1930s immeasurably strengthened the ability of renters to influence housing policy.

All of the essays in this volume are superbly researched. The exciting essay on the Great Depression period by Mark Naison and the lengthy surveys of the post–World War II era by Joel Schwartz and the 1970–84 period by Ronald Lawson are exceptionally thorough. One of the strengths of this volume is the considerable time range that it covers; another is the way in which it is organized. The individual essays read like chapters in a book and contain few of the discontinuities of theme or style that such anthologies usually suffer from. The book is a valuable source for understanding how social movements evolve in response to the changing circumstances of the twentieth-century city. It is an important contribution to social and urban history and deserves a wide readership.

KENNETH L. KUSMER
Temple University

DAVID A. CLARY. *Timber and the Forest Service*. (Development of Western Resources.) Lawrence: University Press of Kansas. 1986. Pp. xiii, 252. $29.95.

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For the last eighty years the U.S. Forest Service has been one of the most visible of the federal land management agencies. Charged with responsibility for the expanding national forest system in 1905, the service has battled with lumber industrialists, rival federal agencies, and in recent years with a broad spectrum of environmental groups to forge a forest policy for the federal timberlands. David A. Clary has plumbed an impressive array of both federal and private collections in preparation for writing this first history of the management of the national forests. As such, this study joins the growing list of books that treat important areas of the agency's responsibilities.

In seven chapters this slim volume traces the evolution and management of the federal woodlands from the turn of the century through the formation of the National Park Service in 1916, the repeated efforts to regulate private timberlands, the market collapse of the Great Depression, the production requirements of two world wars, and the sharply increased demand for national forest timber after 1945. In Clary's view Forest Service officials have acted as single-minded technocrats, as zealous missionaries determined to protect the federal forests for future generations. Isolated from the public and unbending in its resolve to carry forth its sacred mission, the agency, according to the author, provides a "study of a unique bureaucratic culture" (p. xii).

Readers should be aware that this book is not the "sprightly work," the "outstanding contribution," nor the "major addition to the historical literature on conservation" that the dust jacket announces. Clary's Forest Service world is filled with moral crusaders—mostly of the reformist and regulatory type—aligned against pragmatic realists. The villains begin with—who else—Gifford Pinchot, one of those "strident reformers, ceaselessly castigating private enterprise" (p. 16). When Pinchot is dismissed "during one of his outbursts" (p. 21), Henry Graves is appointed chief to heal the wounds. The good chiefs include the likes of William Greeley ("a pragmatic man who attempted only the possible" [p. 71]), who urged cooperation between the government and industry. During the New Deal years and after, "socialistic" crusaders and regulators again headed the agency—Ferdinand Silcox, Earle Clapp, and Lyle Watts.

With the election of Dwight Eisenhower in 1952, however, the Forest Service faced a man "who was averse to regulatory crusades founded on the moral inferiority of American businessmen to federal bureaucrats" (p. 147). The decade of the 1950s, therefore, ushered in change, although the Democratic party continued to "weasel" about the careful management of resources. Truly, this is a presentist tract for the times. If only the world were so simple, and if only the author had consulted recent secondary literature on his subject.

WILLIAM G. ROBBINS
Oregon State University

WILLIAM R. CHILDS. *Trucking and the Public Interest: The Emergence of Federal Regulation, 1914–1940.* Knoxville: University of Tennessee Press. 1985. Pp. xiv, 243. $19.95.

One welcomes the appearance of new monographs on aspects of regulatory experience, especially since historians if affected (as they probably were) by the successive ideological views that regulation was a "good thing" (liberalism) or a "bad thing" (Reaganism) should by now be well beyond such simplicities. The reader of this study of the trucking industry as it came under federal regulation would hope for enlightenment on why the industry was regulated, how, and with what results. This book is best on the second of these questions. William R. Childs makes little effort to evoke the competitive chaos of the industry as it grew in the 1920s and then as depression arrived. It was remarkably easy to enter this line of business, with the usual result that independent operators lowered profits, safety standards, and costs to shippers, prompting efforts at what turned out to be ineffective state regulation. The author appears to believe that the industry nonetheless should not have been nationally regulated at all, but the National Recovery Administration brought this in 1933, and the Motor Carrier Act of 1935 extended it. The trucking industry, only recently and incompletely organized in two main trade associations, was divided on the issue and must be said to have mainly acquiesced in rather than demanded regulation.

The book is most useful as it describes in close detail the regulatory process after 1935—a heroic bureaucratic effort, for truckers were about as easy to bring under control as farmers. This is the "how" part of the story, and Childs confirms the impression of Federal Coordinator and Interstate Commerce Commissioner Joseph Eastman that was established in Earl Latham's *The Politics of Railroad Coordination, 1933–1936* (1959): that he was one of the New Deal's most able and interesting people, a superb regulator who worked to guarantee the profits and viability of established enterprises in American transportation, although he was always a closet socialist. As to the "why," it seems that nobody knew what else to do with trucking. The overall vision of a national transportation policy that animated Eastman and a few other New Dealers, and within which trucking would be tied to rail and water traffic, does not come through clearly.

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And the results? The vantage of 1940 is perhaps too early to formulate firm views on them. Childs candidly tells us that "the state commissioners and the railroad executives . . . gained the least . . . the truckers and the ICC, the most" (p. 172). This seems a judgment as to power, not an economic verdict. At another point he concludes that "by the early 1940s, then, the truckers had achieved a stability in their business that had eluded them since . . . the 1920s" (p. 166), but we are given no definition of "stability" along with other concepts found in the analysis, such as "the common good." Yet the author, after commendable labor in the records of the industry and the ICC, displays an even hand, denying that this was a simple story of capture or a corporatist distortion of either political or economic systems. One might have wanted greater clarity on these and other matters at various points in the book, but the author's ideological detachment has produced a useful volume that readers may to some extent interpret for themselves.

OTIS L. GRAHAM, JR.
University of North Carolina

MARTIN BLUMENSON. *Patton: The Man behind the Legend, 1885–1945*. (Thomas Congdon Book.) New York: William Morrow. 1985. Pp. 320. $17.95.

DONALD SMYTHE. *Pershing: General of the Armies*. Bloomington: Indiana University Press. 1986. Pp. xii, 399. $27.50.

These books reflect the personalities of their subjects. Martin Blumenson's book is episodic, impressionistic, emotional—much like Patton. Donald Smythe's is a scholarly portrait, reflecting Black Jack's devotion to careful preparation.

Patton and Pershing cherished each other and worked long and well together. Patton saw Pershing as mentor, almost father figure; Pershing saw in the brash young fighter someone he might have been.

Blumenson, at pains to show the man behind the Patton legend, points out the lifelong effects of dyslexia: feelings of inadequacy that forced overcompensation. Outwardly confident, Patton often doubted his capacity to meet his own tough standards of performance, craved praise and approval, felt odd superiorities to contemporaries, and fed off his wife's lifelong devotion to his career. Shallow, insecure, but nonetheless skilled in military history and ways of war, Patton ranks among the more complex of great captains. What he did history tells; which of the various Pattons is real remains wrapped in the mystery he created. Often appealing, often repelling, the general baffles, irritates, charms, and teaches much about will and war.

Pershing, too, was a teacher of will and war and of dedication, devotion, persistence, and ambition. Smythe has worked on this book for some twenty-five years; his first volume, *Guerrilla Warrior*, appeared in 1973 and set a high level of scholarly effort that promised a splendid finished product. This concluding volume fulfills the promise.

Smythe's research is superior. Showing uncommon skill in finding obscure sources and developing new uses for familiar ones, he has produced one of the better military biographies. Well-written, carefully critical, fair, Smythe's book will reward professional and popular readers.

FRANK E. VANDIVER
Texas A&M University

MARK T. GILDERHUS. *Pan American Visions: Woodrow Wilson in the Western Hemisphere, 1913–1921*. Tucson: University of Arizona Press. 1986. Pp. xiii, 194. $25.95.

Pan-Americanism is a U.S. vision of reciprocally beneficial economic and political integration among countries and elites of the Western Hemisphere. Progressive, reformist, and liberal ideas and cooperation would allay Latin American mistrust of the United States.

Mark T. Gilderhus's new book is the most complete now available on pan-Americanism from 1913 to 1921. It merits careful reading. Gilderhus treats pan-Americanism as "a kind of centerpiece" (p. xi) of Woodrow Wilson's Latin American policies and relates it to several contexts that help explain the twists and apparent obscurity of this vision. These contexts include often awkward U.S. diplomacy, interventionist imbroglios, use of organizations such as the Bureau of Foreign and Domestic Commerce and Pan American Union, the European war, and Wilson's worries about insurrectionary Mexico.

Gilderhus focuses on U.S. relations with Argentina, Brazil, and Chile. U.S. officials hoped for growth in foreign trade and for stable and profitable opportunities for foreign investment there, and they felt that these three countries would buttress the United States' position in the hemisphere, especially in resolving conflicts. The book well documents Gilderhus's claims that Wilson's pan-Americanism called for "regional integration under [the] United States' leadership" and that outcomes of Wilson's policies "often ran counter to his expectations" (p. xi).

Opening the chronological account, chapter 1 describes the beginnings of pan-Americanism, which sought to improve the image and foreign commerce of the United States. Chapter 2 presents integration and efforts to establish forums and institutions to facilitate financial relations and amity. It treats the unsuccessful pan-American treaty project, friendly conferences on economics and science, and

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the International High Commission, an ineffectual body designed to plot common strategy for commercial, financial, and legal integration.

Two succeeding chapters detail complications arising from the world war and the subordination of Latin American affairs to U.S. interests elsewhere. They argue that Wilson's pan-American vision remained vital. Gilderhus briefly discusses, but does not analyze, conceptual issues arising from scholars' use of dependency and diffusion models to assess advantages and handicaps of integration schemes such as pan-Americanism. He properly weighs North American economic and political motives. He might also have noted an argument that was alluring to all pan-Americanists and that U.S. partisans liked to emphasize—that regional comity would impel economic development analogous to that of the United States. To many, this notion seemed to offer systemic and long-lasting advantages beyond short-term investments, profits, and trade.

The carefully produced volume is generally well written. Some portions of the text, however, seem unnecessarily laden with repetitive quotes and detailed descriptions of primary documents. Other segments are weakened by a failure to make a point clearly or to state a subthesis early in a section in which the evidence is developed. This occurs, for example, in the accounts of the U.S.-British disagreement of early 1914 over U.S. policy toward Mexican leader Victoriano Huerta and of the project for the pan-American treaty. A well-organized bibliography lists the array of U.S. archival and other documents Gilderhus carefully mined. The eight-page annotated discussion of secondary literature refers to the entire Wilson era as well as dependency theory and U.S.–Latin American relations as a whole.

ROBERT N. SEIDEL
Empire State College
State University of New York

FREDERICK S. CALHOUN. *Power and Principle: Armed Intervention in Wilsonian Foreign Policy*. Kent, Ohio: Kent State University Press. 1986. Pp. xi, 333. $28.00.

ARTHUR WALWORTH. *Wilson and His Peacemakers: American Diplomacy at the Paris Peace Conference, 1919*. New York: W. W. Norton. 1986. Pp. xiii, 618. $35.00.

Some American presidents have touched the imagination of contemporaries and later generations, and such is the case with Woodrow Wilson, about whose political career books continue to pour forth. Frederick S. Calhoun has used a literary formula of recent times to offer a reinterpretation of Wilson's ideas on intervention. He begins with a Wilson quotation, of which there are legion. The one he chooses is admittedly characteristic of the man: "The force of America," President Wilson said on June 5, 1914, "is the force of moral principle." Calhoun believes that Wilsonian foreign policy, broadly defined, was "the search for principled applications of power in the relations between nations" (p. x). He analyzes interventions from Veracruz to Vladivostok—Mexico, Haiti, Santo Domingo, the world war, and Russia. These interventions, he writes, were variously to preserve order, promote democracy, uphold international law, and thwart aggression. Wilson later incorporated the lessons of his interventions in the League of Nations Covenant.

The author's reinterpretation appears in broad sweeps, deals with issues that are debatable, and at least to me is both impressive in terms of the author's very considerable investigations and yet somehow fragile in its conclusions. Calhoun's book rests on wide reading, even if almost all the manuscript collections he consulted are in Washington, D.C. The fragility of the conclusions appears in the book's last sentence: "Wilson understood what later generations have tended to forget: it is only through the limitations on force that civilization survives" (p. 267). If not an enigmatic statement, it is an uneasy truism. The author draws the covenant as a testimony to Wilson's experience, and yet by another interpretation the covenant was neither fish nor fowl, neither a development of international law nor an outright alliance, which were the only two directions that experience recommended. On the penultimate page of the book Calhoun develops a point about latter-day American policy: that Wilson's example "teaches the appropriate—and inappropriate—applications of armed power, that force can defend an idea better than it can promote one." Again, Calhoun resorts to enigma, or truism. The comment is indeed almost Wilsonian in its brittleness. The author sees error in the Korean and Vietnam wars, whereas one might contend that Korea made sense (a reliable if autocratic government as an ally, with an army that would fight and a controllable border) and Vietnam did not. He draws the Wilsonian occupations of Haiti and Santo Domingo as blatantly ethnocentric humanitarianism, to use his phrase, and, apparently, he is critical of the League of Nations mandates because they applied Wilson's ethnocentrism on a world-wide scale. But, then, although the parallel is acceptable, the conclusion may not be.

The huge book by Arthur Walworth, his second and concluding volume on the Paris Peace Conference of 1919 (the first, on the conference's preliminaries, was *America's Moment: 1918* [1977]) uses every scrap of manuscript material that Walworth could find, in the United States and abroad. Here is

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an enormous research, preceded by a major biography of Wilson; Walworth has spent many years on his subject. He deals with all the principal issues at the peace conference, and many of the minor ones. In five parts, and twenty-eight chapters together with an epilogue, he traces the conference, from Wilson's first trip to Europe through the many crises and confusions. In remarkably good form he holds the lines of his narrative, and only once does he repeat himself (pp. 201, 212); this alone is an accomplishment. But the book's major accomplishment is to bring together all the facets, the first book that really has done so.

Inevitably, in a book centered on Wilson and his peacemakers, the question is why things went wrong. Surely, as Wilson maintained, the treaty if harsh was also a worthy result, but things went wrong. Walworth does not spare criticism of the president. Wilson worked by himself, and fellow delegates—except for Colonel Edward M. House—knew little. Gradually, House fell from favor, because, Walworth believes, the colonel boasted to newspapermen. With good intentions House talked frankly and freely and thereby perhaps unconsciously played up his own initiative and skill in negotiation. Rumor began to circulate that the colonel's head had swollen. After Wilson returned home in mid-February and came back a month later, he believed House had compromised too much, and the colonel soon was noticeably out of favor. From that point on, the president had to handle everything. Even Wilson's personal staff became fearful of contradicting him. "Such a picture as I could draw of this household!" wrote Ray Stannard Baker in his journal of March 31, 1919.

Pitifully, the president lost control, first in Paris, then in the United States. He especially failed to remember the need of compromise, which is "the law of Society in all things," he once had written. At Paris, Walworth states, he compromised with more impulsiveness than wisdom. He was gradually moving toward a fight with his enemies in the Senate. Walworth writes of how the president's father had been the same way. "This state of mind, which had almost wrecked a theological school in the South and even Princeton University, boded nothing but ill" (p. 295).

Calhoun's book ably draws the interventions—including World War I, which was not exactly comparable to taking over the governments of Haiti and Santo Domingo. Walworth has virtually exhausted the literature on the Paris Peace Conference. He has produced an account that is detailed and accurate. In many places it is hauntingly eloquent. And yet the Wilson literature is by no means finished, as Arthur S. Link and his learned colleagues continue to produce scrupulously edited books in their Wilson papers project, volumes that will inspire more books about Woodrow Wilson.

ROBERT H. FERRELL
Indiana University

BRUCE CLAYTON. *Forgotten Prophet: The Life of Randolph Bourne*. Baton Rouge: Louisiana State University Press. 1984. Pp. x, 275. $25.00.

Once one recovers from the uninspired and uninspiring title, Bruce Clayton's study can be recognized as a serious work of scholarship, superbly executed. Clayton has produced a work that is part of a relatively new genre, "social biography," of which Nick Salvatore's *Eugene V. Debs: Citizen and Socialist* (1982) is another outstanding example. The genre goes well beyond the familiar "life and times" category to treat historical figures as social beings, whose development is largely explained through their interactions with particular places, societies, and times.

Thus, Clayton's portrait of this early twentieth-century radical journalist gives us not just the early development of a persona against a static backgound of turn-of-the-century Bloomfield, New Jersey, but instead a young man and a town that were both, as it were, growing up. One is made to feel the reactions of the cruelly crippled youth to his family's ambiguous status, to family members, ministers, and teachers, and to the full matrix of social messages conveyed by all of them. Yet Clayton sufficiently keeps the focus on his protagonist—his brilliance, his fortitude, his receptivity to vital elements of nurture—to make it no surprise when the diminutive hunchback becomes president of his high school's senior class.

With similar wide-ranging perceptivity, but always without bathos, Clayton describes and assesses Bourne's plunge from bourgeois security to proletarian exploitation in his late teens and then the winning of a tuition scholarship to Columbia. The university, like Bloomfield, is made to come wonderfully alive as Bourne makes his way, both accommodating and resisting, within its formidable grasp. On the brink of World War I a remarkably luxurious traveling fellowship to Europe strengthened the youthful radical's qualifications for an already promising career in political journalism.

Bourne died four years later in the postwar influenza epidemic, having produced a hundred or so articles for general-circulation and radical magazines while collecting many of them into three cut-and-paste books. Bourne was neither especially learned nor profound, as Clayton without condescension makes clear. He had, however, many insightful and novel perceptions that he was able to convey to readers through a vivid, muscular writing

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style. He also grasped early the disastrous implications of the involvement of the United States in the war, earning a place of honor among the several dozen prominent American intellectuals—including President Woodrow Wilson until war came—who felt similarly. Clayton's highly readable yet sensitively and complexly argued work definitively establishes Bourne's place in the still unfolding history of American radicalism.

CHARLES FORCEY
State University of New York, Binghamton

JOYCE ANTLER. *Lucy Sprague Mitchell: The Making of a Modern Woman.* New Haven: Yale University Press. 1987. Pp. xx, 436. $29.95.

This is a competent biography, thoroughly researched from rich primary sources, including personal diaries and oral interviews, of a woman whose contribution to early childhood education in the United States was far-reaching and profound. Lucy Sprague Mitchell was a progressive educator, moving spirit of the Bank Street College of Education (in its various forms), and inspirer and author of children's books. The "Here and Now" series and the Golden Books are products of her fertile brain.

Born to a prominent Chicago family in 1878, Lucy Sprague was one of the heirs to the Sprague Warner wholesale grocery fortune. She graduated from Radcliffe College in 1900 and began her professional career as the University of California's (Berkeley) first dean of women. Sprague had the mantle of Alice Freeman Palmer planted firmly on her shoulders during her college years when she lived with the Palmers, and her position at the University of California was certainly in the Palmer tradition. But her career as a university administrator did not satisfy her. It was not until after her marriage in her early thirties to the noted economist, Wesley Clare Mitchell, that she decided the direction her professional career permanently would take. After an exploratory interlude in New York just before her marriage, she opted not to stay in higher education nor to follow the settlement house model she had just investigated but to look to the schools and early childhood education as a way to reform society.

Joyce Antler sees Lucy Sprague Mitchell as a transitional woman, a bridge between the pioneers such as Jane Addams and Lillian Wald who pursued their careers as single women and the married traditional women, devoted only to family, who formed the nineteenth century's majority. Also, she sees Mitchell breaking with earlier feminist tradition in turning away from feminist political solutions. Suffrage and equal rights were not her primary concerns. Instead, she practiced what Antler calls "feminism as life process," a personal search for autonomy and fulfillment. Although Antler describes and analyzes Mitchell's educational contributions, her real focus is on how Mitchell balanced her commitment to husband and family with her demanding career, a dual role that Antler sees as having great contemporary relevance. Mitchell's money, however, bought not only research funds and school buildings but also servants and work places next door to her home. Few contemporary working mothers can draw on similar resources, and Antler's emphasis on Mitchell's pioneering role as purveyor of a new life style for women is a bit strained. Mitchell's real contribution lies not in the life pattern she developed but in her significance as a progressive educator.

A bibliography and more adequate footnotes would increase the book's usefulness for scholars. The use of initials, rather than abbreviated titles, in the footnotes is annoying as is the practice of making full citations only once, rather than once in each chapter. But these are editorial deficiencies.

Antler's biography makes a real contribution to both the history of women and the history of American education.

RUTH BORDIN
Ann Arbor, Michigan

ANNE C. LOVELAND. *Lillian Smith: A Southerner Confronting the South.* (Southern Biography Series.) Baton Rouge: Louisiana State University Press. 1986. Pp. xii, 298. $22.50.

Lillian Smith has long had a well-deserved reputation as an outspoken opponent of segregation and a courageous supporter of civil rights. Within the last few years, her place in the southern literary tradition has received increased attention, but in each case Smith was merely part of a larger study. She deserved more thorough treatment. Now, Anne C. Loveland has provided this in her fine biography.

As a young woman, Smith recognized a conflict in her nature, which she called her Martha and Mary split. The Martha side represented her conscience, which moved her to forsake her own needs and help others. The Mary side corresponded to her creative and intellectual needs, which led her to writing. Loveland concludes that the tension created by this internal division was a driving force in her life. In 1936 Smith indulged her Mary side when she and Paula Snelling began a journal, *South Today*. The women used this publication to review literature on the South and to voice their opposition to segregation. Smith believed, with her Martha side, that literature could be used to help the South solve its racial problems.

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Segregation, according to Smith, was not just a problem between blacks and whites but a way in which everyone shut out alien ideas, emotions, or experiences. To remedy this, she insisted that people must be shown proper behavior before legislation could be effective. When she published her controversial novel, *Strange Fruit*, she intended the book to be a statement about human relations not just race relations. To Smith's dismay, most critics and the public took the latter view. Subsequent writing over the next twenty years, even on topics other than race, failed to dispel this image.

Dominated by her Martha side, which she said compelled her to clean up the mess in the South, Smith joined or supported a variety of liberal organizations. This led to threats of physical violence and in the 1950s a boycott of her work by many newspapers and magazines. Before her death in 1965, Smith gave up her insistence on solving racial problems by good example and embraced nonviolent protest. She opposed, however, the growing militancy of CORE and SNCC, arguing that her life had been dedicated to breaking down barriers between people, not building them.

The main problem in Loveland's work is organization. The book is predominantly a chronological study, but three of the twelve chapters are more topical in nature. The chapter entitled "Segregation" repeats material found in previous and subsequent chapters. In "Woman Born of Man," Loveland halts her chronological approach and traces Smith's stance on women's rights. The most serious problem occurs in chapter 10, "Relationships," which centers on Smith's private life. Little is found on this in the previous pages. This excellent chapter reveals much about Smith and is crucial to understanding her public persona, but the book would have been strengthened had the information been integrated into the text instead of isolated in this fashion.

Smith wanted "above all to be recognized as a significant thinker and writer" (p. 262). Although her stature as a writer has risen recently, her goal will probably never be realized, because, as Loveland notes, her "philosophical thinking was generally derivative and her literary effort unexceptional" (p. 262). Her significance lies in her long and brave fight against segregation. Although Smith would be upset, this is a splendid legacy, and Loveland's rich portrait is a fine tribute to this remarkable person.

GARY L. HUEY
Ferris State College

JOSEPH M. MCSHANE. *"Sufficiently Radical": Catholicism, Progressivism, and the Bishops' Program of 1919*. Washington, D.C.: Catholic University of America Press. 1986. Pp. x, 307.

The American Catholic bishops' recent pastoral letters on peace and the economy have commonly been viewed as major innovations, but historians of Catholicism are well aware of the long tradition of social teaching by the Roman Catholic hierarchy in the United States. Joseph M. McShane's book examines the origins and significance of a major landmark in that tradition, the Bishops' Program for Social Reconstruction of 1919, which startled American Catholics and others by calling for, among other things, a minimum wage, government housing for workers, social insurance, and worker participation in the management of industry.

McShane recognizes that the Bishops' Program was significant not so much for its content, which was standard Progressivism, but because it was Catholics who were advocating such things. Working-class Catholics and their bishops had long remained aloof from Progressivism, with its tone of middle-class uplift and thinly disguised Protestant moralism. Yet a growing number of socially alert Catholics recognized the essential justice and necessity of many Progressive social-justice measures and struggled to reconcile them with official Catholic teaching. McShane provides a close biographical account of how a learned Minnesota priest-reformer, John A. Ryan, effected his stimulating intellectual marriage between American-style pragmatic liberal reform and the scholastic neo-Thomism of European social Catholicism, as set forth in Pope Leo XIII's 1891 encyclical *Rerum Novarum*.

The historical puzzle has always been how Ryan and his allies were able to win the backing of the hierarchy for their advanced proposals. McShane provides a persuasive answer by showing that World War I created a new sense of Catholic unity and confidence and convinced the most influential bishops that advocacy of reform was a way to assert an American Catholic identity while promoting religious principles. In the process, the bishops demonstrated just how American they were becoming when they hired a Madison Avenue advertising agency to publicize their cause, with spectacularly successful results.

Although it occasionally bogs down in the minutiae of the program and its reception, the book presents a highly lucid account of an important development in American Catholicism. Like the Bishops' Program itself, McShane's history stands solidly within the tradition of liberal Catholic thought. Perhaps, with a bit more intellectual distance from his subject, the author might have exanded his insights into the strong relationship that apparently developed between liberal social reform and American identity for some American

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Catholic elites. McShane's analysis could potentially explain the weaknesses as well as the strengths of a Catholic liberalism that has sometimes aroused the suspicion of the Vatican and some Americans.

MEL PIEHL
Valparaiso University

RICHARD S. KIRKENDALL. *History of Missouri.* Volume 5, *1919 to 1953.* Columbia: University of Missouri Press. 1986. Pp. viii, 438. $28.00.

During 1986 two major books on Missouri were published: the one under review, which laments the slow development of the state, and David Thelen's *Paths of Resistance: Tradition and Dignity in Industrializing Missouri*, which celebrates Missourians who resisted economic and social development in the period 1877–1917. That Richard S. Kirkendall's volume deals with the period 1919–53 has much less to do with the difference in interpretation of the state's history than the viewpoints of the two authors and the state's complexity. Both see Missouri in a state of decline during the periods they treat, but Thelen writes that decline set in with the spread of a market economy and the destruction of traditional society, whereas Kirkendall thinks that traditional attitudes toward taxes, politics, and education hampered development, which kept Missourians from experiencing the quality of life they deserved. However interesting debate over the two interpretations might be, the focus in this limited space must be on Kirkendall's book.

Besides emphasizing the theme of slow growth for Missouri between 1919 and 1953, Kirkendall discusses the rise of Harry S. Truman to the presidency. The author attempts to weave these themes together in the book's twelve chapters, but the transitions are not always smooth. After introducing his themes in the first chapter, Kirkendall devotes three chapters to the 1920s; three more to the Great Depression, the New Deal, and the fall of the Pendergast machine; two chapters to World War II; and individual chapters to life at midcentury, politics at midcentury, and Truman as a Missourian in the presidency. Kirkendall handles the final chapter with great skill. Although the book is not footnoted, which follows the series's format, the author frequently mentions who he is citing in the text and has written an excellent essay on sources.

To argue the state's slow growth, Kirkendall uses statistical material from the various censuses and other sources to compare Missouri with other states and the nation. Although reading a host of numbers is sometimes tedious, interesting data also emerge, and the book is filled with fascinating information about such things as the percentage of farm homes with electricity from one period to another and the percentage of houses in St. Louis without indoor plumbing in 1950. The author succeeds in demonstrating that Missouri lagged behind the nation in various categories and seriously failed to keep up with such rapidly developing states as Florida and California.

Kirkendall attributes Missouri's failure to a complicated political system with divided power and political leaders with limited visions, an underfunded educational system and educational leaders who demanded far too little, a system of segregation that began having cracks in it but was not dynamically attacked, and, underlying all of the above, a low-tax mentality and the state's complexity. Although he frequently mentions these two ideas, Kirkendall fails to give much attention to explaining either concept. Instead of what he sees, the author wishes for "heroic leaders," "ones that challenged their culture more than they conformed to it" (p. 7).

Strengths of the book include a very good discussion of blacks, space devoted to women, immigrants, and labor, an enjoyable and informative treatment of jazz and baseball, and an able discussion of Truman and the Pendergast machine. Weaknesses of the book include the failure to develop the crucial concepts of the state's complexity and its low-tax mentality, as mentioned above, the lack of flesh and blood on the cold statistics, and too little treatment of outstate Missouri, particularly the changing pace of life in the rural areas as automobiles, tractors, and electricity became important in the countryside. So, although this is a good book on Missouri between 1919 and 1953, a definitive volume about those decades remains to be written.

LAWRENCE O. CHRISTENSEN
University of Missouri, Rolla

RONALD EDSFORTH. *Class Conflict and Cultural Consensus: The Making of a Mass Consumer Society in Flint, Michigan.* (Class and Culture.) New Brunswick, N.J.: Rutgers University Press. 1987. Pp. xvi, 294. Cloth $40.00, paper $12.00.

"Twentieth-century labor history continues to be written almost as if mass consumerism and automobility never happened," Ronald Edsforth tells us, "as if these things could have developed without simultaneously transforming the traditional values that informed working-class protest and organization" (p. 30). This is surely an overstatement; ever since Robert S. Lynd and Helen Lynd took the pulse of "Middletown" in the mid-1920s, the seductive power of the Ford and the Philco has been a staple of social commentary and of popular historical analysis. We still know rather little, however,

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about the ways in which the lives and values of industrial workers were changed by their increasing access to consumer goods and especially to the automobile. It is around this important but inevitably diffuse topic that Edsforth has organized his study of Flint, Michigan, in the years since 1900.

Twentieth-century Flint is an automobile town; General Motors (GM) was born there in 1908. Its growing industrial work force was relatively affluent, and the city itself—its physical form, its patterns of sociability—was shaped early on by the automobile. Flint's workers, therefore, were pioneers of a sort: they were among the first working-class inhabitants of the automobile-centered consumer culture that has come to dominate the Western world. And they possessed, according to Edsforth, a radically new consciousness, fully evident in the politics of Flint by the early 1920s. Responding to the blandishments of consumerism, Edsforth argues, Flint's workers turned their backs on communal values, opting instead for privatized lives and the ideal of an "enlarged material self" (p. 66). As they did so, they surrendered control of their town to a conservative business elite.

Economic collapse in the 1930s roused Flint's workers from their political torpor. They revitalized the Democratic party locally, and a minority among them were part of the ultimately successful drive to unionize GM. A revivified sense of class solidarity was evident in Flint by then, according to Edsforth, and it persisted into the 1940s, only to be contained and then greatly weakened by the company and the union. Edsforth holds the emerging bureaucracy of the United Auto Workers particularly responsible here, lamenting its collaboration with GM in destroying the informal shop steward system that had grown up in Flint's plants in the late 1930s. But he must logically look to the postwar prosperity for a major part of the explanation. "Ultimately . . . it was the successful restoration of the mass consumer economy upon which the whole political culture of the Cold War years in Flint rested" (p. 216).

Nothing in this summary will surprise American historians. Despite its prominence on the frontier of mass consumerism, Flint seems to have mirrored larger national moods. And that is precisely the problem. Edsforth has given us a vigorously written account of Flint's industrial and political development, of changes in the nature of auto work, and of the organizing drives of the 1930s. But he has not gone much further than the Lynds went in demonstrating the effects of mass consumerism on workers and their communities.

LESLIE TENTLER
University of Michigan, Dearborn

RICHARD DRINNON. *Keeper of Concentration Camps: Dillon S. Myer and American Racism*. Berkeley and Los Angeles: University of California Press. 1987. Pp. xxviii, 339. $24.95.

Although many have noted ironically the progress of Dillon S. Myer (1891–1982) from the directorship of the War Relocation Authority (WRA), which incarcerated Japanese Americans, to the commissionership of the Bureau of Indian Affairs (BIA), it has taken the imagination of Richard Drinnon to connect these episodes in a coruscating work of scholarship and anger. Anyone who still believes that the WRA (1942–46) was an agency of benign incarceration or that the BIA under Myer's stewardship (1950–53) acted in the interest of Indians needs to consider the ugly and indisputable facts of torture, degradation, and persistent denial of basic human rights recounted here. This review will concentrate on the WRA years. Using a wide variety of sources, including Freedom of Information Act material from files of the Federal Bureau of Investigation, Drinnon has examined the darker side of Myer's two empires in more detail than has anyone else. All students of civil liberty are in his debt for the uncompromising way in which he exposes the shameful wartime collaboration of Roger Baldwin and most leaders of the American Civil Liberties Union with the government whose abuses they were supposed to oppose.

Yet the work is badly flawed and suffers from serious imbalance. Rather than a biography, it is more like a literary mugging, a mugging that even extends to Myer's ancestor. After describing an all-too-typical 1800 killing of an Indian in what became Licking County, Ohio, where Myer was born and raised, Drinnon writes that the "respect for Indian-haters was so pandemic we may simply assume, in the absence of evidence to the contrary, that it was shared by Dillon Myer's grandfather" (p. 12), Jacob, who moved there thirty-five years after the event.

Even more serious is the lack of any significant discussion of what wartime America was like, a context so brilliantly delineated in John Dower's *War without Mercy* (1986). Although racism seems all but eternal, Myer and his WRA associates need to be seen as part of a continuum of attitudes toward Japanese Americans. Unlike many Americans, those officials wanted to integrate inmates whom they regarded as "good Japanese" into American society. To suggest—as Drinnon does—that Myer felt that "nonwhites" had "no rights which the white man is bound to respect" (p. 269) is unfairly to place him in the same category as contemporary Taneyites John Rankin and A. B. "Happy" Chandler. Similarly, Drinnon's criticisms of the Japanese American Citizens League (JACL)—criticisms that echo those

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made contemporaneously by San Francisco attorney Wayne Collins—though largely valid, are made without in any way attempting to understand the kinds of problems and pressures that its young Nisei leaders faced. To understand their problems is not to pardon the JACL leaders. I agree with Drinnon that some of what they did was unpardonable. But it seems to me that they, too, were victims of American racism and wartime hysteria.

Notwithstanding these and other caveats and the imbalance within it, this is a work that every student of racism in modern America will have to study and consider.

ROGER DANIELS
University of Cincinnati

ROBIN EDMONDS. *Setting the Mould: The United States and Britain, 1945–1950*. New York: W. W. Norton. 1986. Pp. xxx, 349. $22.95.

The last several years have seen a veritable explosion of European scholarship on the diplomacy of the early cold war, the latest contribution to which is this very readable book by Robin Edmonds. Edmonds's subject is the Anglo-American relationship in the years between 1945 and 1950, arguably the most important bilateral relationship in the so-called free world of that day. The book will be very useful to American diplomatic historians, not because it tells us something new but because it gives a British point of view, specifically that of a British foreign service official who was associated with some of the issues discussed and who has carefully reinforced memory with a close scrutiny of the documents available in the Public Records Office.

In his survey of the Anglo-American relationship, Edmonds includes sections on the Middle East, China, and Southeast Asia, on atomic energy policy, and on such initiatives as the Marshall Plan and North Atlantic Treaty Organization. On these last two initiatives he takes a position similar to that of contemporary British and European "revisionists," who give the British much of the credit for getting the Marshall Plan off the ground, for launching the negotiations that led to the North Atlantic Alliance, and for sustaining these negotiations until they bore fruit in 1949. His account squares neither with the works that have described the Anglo-American relationship as special nor with those that have portrayed it as ambiguous or ambivalent. But it comes nearer to the second group than to the first. Although the relationship was special in that the British and American governments dealt more closely with each other than with other governments in the non-Soviet world, it was nonetheless marked by controversy at every turn.

Edmonds chronicles the breakdown of wartime collaboration between the two powers in the field of atomic energy, as well as their differences in the Middle East and Asia, and he notes how Britain's opposition to Anglo-European economic integration marred Anglo-American relations in an area in which the initial prospects for cooperation had seemed to be good. By the early 1950s, moreover, the bipolarization of the world and the global mission of the United States had inevitably led to an Anglo-American relationship in which Britain functioned as the "principal ally" of the American superpower (p. 253). Thereafter, according to Edmonds, the relationship became progressively more attenuated as the two countries clashed in the Suez crisis of 1956, as Britain's capabilities continued to decline, and as the United States preferred to act more unilaterally than before or in closer cooperation with such ascendant allies as West Germany and Japan.

Edmonds mixes a pithy narrative with critical, but balanced, judgments that are altogether fair. He castigates the British for their wrong-headed policy toward Europe, and the British and the Americans for their failures in the Middle East. A sense of missed opportunity also permeates his account, particularly his discussion of the prospects for international control of the nuclear arms race. So does a sad recognition that Britain has not been able to reverse the "remorseless decline" (p. 248) that has robbed it of great-power status and thus of the hope for a special relationship with the United States.

MICHAEL J. HOGAN
Ohio State University

DAVID ALLAN MAYERS. *Cracking the Monolith: U.S. Policy against the Sino-Soviet Alliance, 1949–1955*. Baton Rouge: Louisiana State University Press. 1986. Pp. 176. $22.50.

David Allan Mayers reconsiders the intellectual formation of American foreign policy toward the People's Republic of China from 1949 to 1955. He argues that Secretaries of State Dean Acheson and John Foster Dulles entertained an analysis of China that adopted Tito's split from the Soviet Union as a model for approaching China. Their assumption was that could promote an independent China against the hegemony of the Soviet Union in Asia. In the late 1940s and early 1950s George F. Kennan proposed that the United States be conciliatory to China. The Chinese leaders would realize that working with Washington was in their self-interest. During and after the Korean War, U.S. tactics changed but the strategy was the same: isolate China and thus push it deeper into the arms of the Soviet Union. The Soviet Union would feel the burden and push

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China away. U.S. propaganda directed toward China called on the Chinese to recognize that the Russians were not living up to their economic and social obligations to the Chinese government. Mayers suggests that, if it were not for the anti-Soviet and anti-Communist fervor that swept the American public and government officials, the United States could have played the China card soon after Mao Tse-tung's rise to power rather than waiting for President Richard Nixon's trip to Peking in 1972.

Mayers's book retraces the tortuous history of U.S.-Chinese relations. The book provides new insights on the Korean War, General Douglas MacArthur's military policies, and the Geneva Conferences. The narration draws on newly released files from the State Department.

The main problem of the book is the Tito analogy. Mayers does not explain the basis of Titoism, nor does he fully analyze the appropriateness of applying the analogy to China. Furthermore, the study does not take into account the larger context of East Asian foreign relations.

Mayers's historical account can serve as an excellent corrective to those who blindly accept the political myths and truisms regarding monolithic Communism and the threats of the Soviet Union and its allies. Mayers reveals that a flexible strategy for dealing with China was in the making, but it was thwarted by public opinion that was uninformed and sometimes hysterical.

RICHARD C. KAGAN
Hamline University

JOHN PRADOS. *Presidents' Secret Wars: CIA and Pentagon Covert Operations since World War II.* New York: William Morrow. 1986. Pp. 480. $22.95.

Are covert activities appropriate for a democratic nation? Are they legal? Are they constructive? John Prados maintains that the time is correct to address these questions because they "are best considered calmly and not in the heat of another intellegence fiasco" (p. 10). Fortunately for Prados, he is wrong. The Iran-Contra debacle underscores the importance of this investigation. The evidence he presents, nevertheless, offers little hope that future administrations will rely any less on clandestine projects than have past ones. Covert operations have become a fundamental feature of U.S. foreign policy, not a supplement to it.

Although Prados delves into such lesser-known operations as those in Indonesia and Tibet, in the main he covers familiar terrain and heavily relies on published materials without discriminating among them. Still, he deserves credit for pulling together an impressive list of disparate sources to produce the most comprehensive survey to date. His evaluation of the National Security Act of 1947 makes clear that neither Frank Wisner's Office of Policy Coordination nor its successor organs ever held clear statutory authority to conduct clandestine operations. Because of individual initiative, supportive presidents, complacent congresses, and a trusting public, they did so anyway. Only the combination of Vietnam and Watergate slowed the momentum, but Ronald Reagan reversed that, at least until now.

The most valuable aspect of Prados's book is its assessment of the secret warriors' record. Although the CIA's targets rapidly expanded from Eastern Europe to encompass the globe and its budget and staff increased commensurately, it consistently failed to achieve its objectives. The triumphs in Iran and Guatemala were exceptions, and Pyrrhic ones at that. Especially for the Third World, the letters "CIA" serve as an abbreviation for U.S. foreign policy, connoting underhandedness, shortsightedness, and ineffectiveness. Reagan's blunders are unique only in that the administration has all but abandoned any pretenses of "plausible deniability."

Because of institutions designed to shield the president, Reagan's complicity in the Iran arms scandal may never be fully disclosed. Yet readers of this volume will surely conclude that his culpability goes well beyond current protestations. A byzantine bureaucracy can provide great latitude, but at no time has the CIA operated outside presidentially established parameters. It is not, and never has been, a "rogue elephant."

Prados argues that covert activities violate democratic principles, are probably illegal, and generally do not work. His prescriptive conclusion, however, is superficial. Prados advocates the enactment of a detailed charter to rein in the CIA, the establishment of a Pentagon-based expert force with exclusive responsibility for conducting paramilitary campaigns when they are vital to U.S. interests, and increased executive accountability. But such reforms cannot rectify a foreign policy outlook plagued by misperception, illusion, and amateurism. The activities of the CIA are a symptom of diplomacy gone awry, not the cause.

From this perspective the book is inherently flawed. By focusing only on the CIA's covert operations, Prados has isolated the implementation of American foreign policy from its conceptualization and formulation. Covert operations fail because of a concomitant failure in the gathering and analysis of intelligence, often rooted in ethnocentric myopia and ideological rigidity. Product cannot be separated from process. The problem for Prados is that he put the cart before the horse.

RICHARD H. IMMERMAN
University of Hawaii

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NGUYEN TIEN HUNG and JERROLD L. SCHECTER. *The Palace File*. New York: Harper and Row. 1986. Pp. xiv, 542. $22.95.

Nguyen Tien Hung and Jerrold L. Schecter describe their work as a study of the relationship between the top officials of the American and South Vietnamese governments during the last eight years of the Vietnam War, but it is also a perspective on the war from the point of view of Saigon's presidential palace. Secret letters from Presidents Richard Nixon and Gerald Ford to South Vietnam's President Nguyen Van Thieu written between 1971 and 1975 form the book's heart. The authors recount in painstaking detail how commitments made in writing by Nixon and Ford were scuttled after America withdrew from Vietnam, abandoning the war and its client state. When Hung, an erstwhile South Vietnamese official, learned of the existence of the letters, hidden in Thieu's secret palace file, he persuaded Thieu to release them as a final lever to pry additional aid from Congress. Although the letters reached Washington too late to stimulate Congress to further generosity, their publication *in toto* here illuminates the crassness and unscrupulousness of the imperial presidency. The letters provide few surprises, since it has long been known that Thieu totally relied on presidential promises that America would reenter the war if and when North Vietnam violated the Paris Peace Accords. The more interesting question is why, and this work is unconvincing. Hung, whose function was to advise Thieu about the Americans, was educated in America, a college professor, and married to an American. His colleague Schecter, a journalist, alludes to the Confucian heritage, the veneration of the wise ruler, and Thieu's inability to see that Nixon could not manipulate the public as indefinitely as he could.

The authors make no attempt to hide their anger and sense of betrayal. Their case is weakened by their overreliance on secondary works with a neorevisionist perspective, including memoirs and interviews with the principals. Although Nixon refused to be interviewed, Hung and Schecter spoke with Thieu twice, as well as with numerous lower officials in both governments. The vignettes based on these interviews provide texture and substance to the story of betrayal. The authors contend that the United States betrayed Saigon, forcing it to sign a flawed accord that enabled Hanoi to win, and claim that with additional aid South Vietnam could have won. The authors manage to make Washington and Saigon almost interchangeable and the players all-American in outlook. There is little insight into the workings of South Vietnamese politics and much denigration of the liberal opponents of the war.

The result of this collaboration is a readable pastiche of scholarly journalism, useful for the extensive interviews and the historically important letters (copies of which exist only in the closed files of the Nixon and Ford administrations). Its utility is diminished by an almost total reliance on right-wing secondary writings and a lack of archival research. The extensive use of created dialogue and descriptions of the principals' thoughts and feelings at key moments make it readable for a general public but limit its usefulness for scholars, as do some factual errors. The domino theory is identified as the containment of China, and we are told that the Soviet fleet refueled at Camranh Bay in 1905 on its way to defeat by Japan. Saigon's story is well told but no more credible in 1988 than it was a decade earlier.

SANDRA C. TAYLOR
University of Utah

DONALD L. FIXICO. *Termination and Relocation: Federal Indian Policy, 1945–1960*. Albuquerque: University of New Mexico Press. 1986. Pp. xviii, 268. $27.50.

The most charitable explanation for the creation of Indian reservations is that the indigenous population that had managed to survive European colonization was to be converted to Christianity, educated, "civilized," and eventually assimilated into American society. Reservations were considered temporary, something that would disappear with the inevitable assimilation of the Indians. Although thousands of Indians assimilated into American society over the years, by the end of World War II around two hundred fifty reservations still remained throughout the United States.

At this point in history Donald L. Fixico picks up the story and describes the U.S. government's ill-fated dual effort to terminate the federal trust status of Indian reservations and relocate Indians to the nation's cities. According to Fixico, Washington policy makers advocated termination and relocation because they felt that the isolated life of Indians on reservations was inhibiting their progress toward assimilation. Noting that all Indians were American citizens (since 1924), that many had honorably served in the armed services during World War II, and that many tribes were both willing and able to manage their own affairs, they argued that the time had come for Indians to be freed from the smothering and costly paternalism fostered by the Bureau of Indian Affairs (BIA).

In addition to a broad survey of Indian termination policy, Fixico details the work of the Indian Claims Commission, which was created to settle all treaty-based claims various tribes had against the U.S. government. He recounts the often tragic personal histories of Indians who opted to relocate to America's cities. Finding themselves wholly unprepared for urban life, many of them either became

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the newest poverty stricken residents of slum housing or returned, disillusioned and destitute, to their former reservations. Fixico effectively points out that, at the same time that the advocates of Indian termination and relocation were justifying their policies as a way to eliminate the BIA, the budget of the bureau grew by leaps and bounds, especially in the 1950s.

This study is a thorough, though not always tightly organized and edited, account of federal Indian policy from 1945 to 1960. The author largely confines himself to a description rather than an interpretation or explanation of the material covered by his research. People with an interest in, but little knowledge of, the recent history of American Indian affairs would benefit from reading this book. The jacket design of a tepee with a city skyline in the background is a striking image of the contrast between traditional Indian and modern American cultures.

Indians' self-determination of their own affairs and future is the publicly stated objective of Indian policy in the 1980s. To lessen the dependence of reservation Indians on the federal government (free the Indians?), many policy makers advocate accelerated cutbacks in funds for Indian health and welfare programs and further decreases in funds used to stimulate economic development on Indian reservations. This line of reasoning suggests that if we peel away the rhetoric that shrouds the policy of Indian self-determination, we may find that the objective of Indian assimilation achieved by the termination of the federally protected status of Indian reservations is still an important current in the meandering but relentless course of American Indian affairs.

NICHOLAS C. PEROFF
University of Missouri, Kansas City

FAY G. COHEN *et al. Treaties on Trial: The Continuing Controversy over Northwest Indian Fishing Rights*. Foreword by ANDY FERNANDO. Seattle: University of Washington Press, for American Friends Service Committee. 1986. Pp. xxvi, 229. Cloth $20.00, paper $9.95.

Fay G. Cohen and her coauthors present a summation of litigation concerning Indian fishing rights along the Columbia River and in the coastal and Puget Sound waters of Washington State since the momentous Boldt decision of 1974, which reinforced and restated those rights in present-day terms. Federal District Judge George Boldt not only reestablished the validity of treaties negotiated between the federal government and certain tribes in the 1850s, granting the tribes perpetual fishing rights, but decreed that the Indians were entitled to one-half the catch.

Since the rise of commercial fishing to a high place in the economy of the region, white fishermen had captured the far greater share of the fish, not only greatly impairing the food resource of native Americans but injuring the indigenous cultures dependent on it. Judge Boldt's verdict, subsequently upheld by the Supreme Court, gave a disproportionate share of the fish to the relatively small Indian population and struck at the heart of both the enterprise of white fishermen and the sport of steelhead fishing enjoyed by whites. An earlier work, *Uncommon Controversy: Fishing Rights of the Muckleshoot, Puyallup, and Nisqually Indians* (1970), by The American Friends Service Committee carried the story down to 1970, detailing the sometimes violent competition and acrimonious litigation in which Washington State constantly defied federal authority over Indian tribes, particularly their fishing rights. The present book carries the story from 1970 to the present.

This work is a valuable addition to the bibliography on federal-state relations and on the continuing role of protector that the federal courts maintain over Pacific Northwest Indian tribes. Boldt did far more than divide the fish harvest. He recognized that it would be necessary to renew and increase the fishing resources and decreed that Indians join with state and federal authorities in building hatcheries, managing the resource, and working together to improve the habitat of all people.

The story that unfolds describes rehabilitation of tribal culture, government, and the economic well-being of the participating Indians. It also reveals the economic benefits derived by the whole society when one of its minorities gains a significant measure of prosperity.

In this well-researched and well-written account, the outcome remains unclear despite an air of optimism. Many questions remain unanswered, but Cohen's book provides hope and suggests measures to be taken in the future.

GEORGE A. FRYKMAN
Washington State University

CANADA

NEIL MACKINNON. *This Unfriendly Soil: The Loyalist Experience in Nova Scotia, 1783–1791*. Kingston: McGill-Queen's University Press. 1986. Pp. xii, 231. $27.50.

There are few who have written on the Canadian loyalists, including myself, who have not paid tribute to Neil MacKinnon's thesis on the loyalists of Nova

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Scotia. Its appearance in print in revised form is therefore very welcome. The story was a difficult one to tell because, unlike the loyalists of New Brunswick and Upper Canada, the Nova Scotian loyalists moved into a partly settled society with an established government and a ruling oligarchy.

The title, *This Unfriendly Soil*, is well chosen, for the loyalists with their high hopes of building a new, better, and loyal New England in the north were to be disappointed. There was a limited supply of good arable land, a limited supply of government subsidies, and an indifferent and often hostile established population.

The volume is organized logically in ten chapters tracing in detail the pattern of loyalist settlement, their early progress and failures, such as the attempt to turn the settlement of Shelburne into a viable city, and the reaction of loyalists to their new environment. Chapter 8, dealing with the loyalists in the Sixth Assembly, will be of most interest to the general reader and should be read by all students of North American political history. The loyalists, who opposed revolution in their land of origin, found themselves shut out of political life by the local oligarchy and, working within the established constitution, formed the opposition. The loyalists were a highly political people. Had they been otherwise, they would not have resisted the revolution.

As MacKinnon points out, the greatest problems of the resettled loyalists were economic. The Nova Scotian economy at that stage of development could not provide the opportunities that these people of drive and energy required: hence the flight of a few to Upper Canada and many back to the United States. The loyalists had arrived in Nova Scotia nursing resentment against those who had driven them into exile. As old grievances gave way to new, the loyalists tended to lose their identity as loyalists.

There is certainly enough here for one book, especially one that deals with the early years of settlement, but the cultural dimension of the loyalists in Nova Scotia receives little attention from MacKinnon. The seeds of Nova Scotian culture, which flourished in the early nineteenth century from the pens of Thomas Chandler Haliburton and others, were planted by the loyalists.

In the epilogue MacKinnon leaves an impression of gloom and defeat, justified by the misfortunes suffered by the loyalists in the early years. Yet those who survived and remained demonstrated, along with loyalists in other provinces, that there was a liberal and ultimately democratic alternative to the republican democracy founded by revolution.

HEREWARD SENIOR
McGill University

PAUL ROMNEY. *Mr. Attorney: The Attorney General for Ontario in Court, Cabinet, and Legislature, 1791–1899.* Toronto: University of Toronto Press, for The Osgoode Society. 1986. Pp. xiii, 396. $45.00.

This suggestive and wide-ranging study investigates much more than its dry title suggests. Paul Romney uses the evolving office of the attorney general to examine a variety of important issues: the political and legal culture of nineteenth-century Ontario, the ideology of the rule of law, and the ways in which that ideology and its institutions were at the service of particular social groups. He does so in often very effective style by concentrating on a series of thematic studies revealing the most crucial changes and most significant controversies in which the attorney general of Upper Canada–Ontario was involved.

The role of the legal institutions and officers in maintaining the power and influence of the established elite is best developed in the analysis of the early colonial period. Here the attorney general is employed as a central figure, as he indeed was, in the machinations of a small group of officeholders to retain power. Romney refreshingly emphasizes the ways in which legal devices and ideology were at the center of this struggle. The problem is that the analysis of the class-based nature of this struggle is only superficially explicated. Who, for example, was "the ordinary Upper Canadian" who resented the ways in which the legal system was operating (p. 189)? It is never quite clear just which classes (as opposed to individuals) were articulating their opposition to the existing legal regime. Certainly, the author eschews any attempt to see this contest (or at least some aspects of it) as between an established elite and an ambitious young bourgeoisie. Similarly, his later view that the famous post-Confederation contest between the legal forces of Ottawa and Toronto was one between competing alliances among the bourgeoisie is only briefly alluded to.

Romney brings to the reader's attention just how often opposing political and social forces used the ideology of the rule of law to serve their own particular interests. The law is revealed as an instrument of power particularly vulnerable to the demands and needs of individuals and of the dominant social class. But the hegemonic function of the law is only briefly and indirectly discussed.

Nevertheless, Romney's study raises some vitally important issues that ought to be at the center of debate among legal historians. This well-written and stimulating book should play an important role in ensuring the centrality of those questions.

JAMES G. SNELL
University of Guelph

BRIAN YOUNG. *In Its Corporate Capacity: The Seminary of Montreal as a Business Institution, 1816–1876.* Kingston, Ont.: McGill-Queen's University Press. 1986. Pp. xix, 295. $30.00.

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The Sulpicians, a religious order, were the recipients of several seigneuries, including Montreal, the most important commercial center of New France, later the colony of Quebec. The subject of Brian Young's work is the development of the order from a feudal to a capitalist institution.

The early development and the postconquest status of the order's land grants are sketched in. The author then describes in detail the administration of the Sulpicians' holdings and notes the increasing specialization similar to that of a business organization.

A problem for the order was its corporate status after the conquest by the British: to what extent were feudal rights compatible with a developing economy, especially regarding milling rights? These rights, by law, belonged to the seigneurs, but several British businessmen challenged these feudal privileges. This particular problem was solved following the rebellions of 1837–38, when the special commission governing the colony granted the order an official corporate status. (It would have been interesting to know if the female religious orders, which also were feudal landowners, faced similar problems.)

Within a Marxist framework the author concentrates on what he chooses to call capitalist development, the move from a simpler form of land dues exploitation to a policy of investment and returns. This diversification is well supported by interesting statistical data, which are, however, given in several forms of currency, and we are not told if we are dealing with current or constant currencies. When the seigneurial system, as it was called in Quebec, was abolished in 1854, revenues were then earned either on the basis of a commutation or a form of interest (*rente*) if the individual land tenant could not pay the commutation fee.

The evidence clearly indicates that the Sulpicians' increases in revenue were invested in a variety of economic endeavors and were profitable. However, conditions were placed by the government on the use of these revenues: some had to be placed in such a way as to assist in the development of charitable institutions and schools. One is led to wonder if a secular feudal lord had conditions placed on the revenues received from commutations or interest. Were all feudal lords equal?

On the whole the work is extremely informative. A few items could have been expanded. Finally, one wonders if the Marxist terminology is really necessary or even valid as an explanatory factor.

CAMERON NISH
Concordia University

BEN FORSTER. *A Conjunction of Interests: Business, Politics, and Tariffs, 1825–1879*. (The State and Economic Life, number 8.) Toronto: University of Toronto Press. 1986. Pp. 288. Cloth $35.00, paper $15.95.

Ben Forster traces the history of tariff legislation and implementation in British North America and Canada from the 1820s, when trade relations with Britain were significantly modified, to 1879, when Prime Minister John A. Macdonald and his Conservatives introduced the National Policy tariff. Forster's book relates to a topic that was crucial to the economic well-being of Canada during this time period. Trade relations including tariffs were a major determinant of economic growth and development of a country that supplied raw materials and food to Britain and the United States and purchased finished manufactured articles from those same countries.

The book's ten chapters divide more or less arbitrarily at 1867 with the first three dealing with the years before that date and the next seven with the period from Confederation to 1879. Separate chapters are devoted to the imperial connection with Britain under the preferential trading rules of the Corn Laws and the timber duties, the Reciprocity Treaty of 1854, and the tariff changes that led up to Confederation in 1867. In the parts of the book that look at tariff making and tariff changes after 1867, a chapter is devoted to the generally prosperous period from 1867 to 1872, after which two chapters deal with business conditions after the onset of the depression that began about 1873; Forster demonstrates the change in business attitudes toward protection that appeared and grew in the 1870s. This is followed by a discussion of the actions of the Liberal government from 1874 to 1878 and the reactions of the business community and of opposition politicians to the government's policies. Finally, two chapters expand on the issues and groups that combined to return the protectionist Conservatives to power in 1878 and then to introduce into law the relatively high tariff changes of this new government.

Forster's book makes an important contribution to the literature on Canada's tariff history because he is able to link the attitudes and beliefs of the business, commercial, and agricultural interests with the policies that political parties both in power and in opposition were advocating and implementing. The reader comes to understand the pressure groups that worked on the politicians to mold tariff policies into particular shapes. The author successfully shows the factors that changed businesspeople's thinking about the tariff and the interaction with the goals of the main political parties of the time. Thus, the reader becomes aware of the political dimensions of the economic policy of tariff protection.

WILLIAM L. MARR
Wilfrid Laurier University

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RONALD G. HAYCOCK. *Sam Hughes: The Public Career of a Controversial Canadian, 1885–1916*. (Canadian War Museum Historical Publications, number 21.) Waterloo, Ont.: Wilfrid Laurier Press; distributed by Humanities Press, Atlantic Highlands, N.J. 1986. Pp. xii, 355. $45.00.

Ronald G. Haycock's book is the first scholarly study of the public career of Canada's minister of militia and defense during World War I. Hughes was born in Durham County, Ontario, in 1853. His public career began in 1885 when he gave up school teaching to purchase a local Conservative newspaper in Lindsay, Ontario, the *Victoria Warder*. Hughes used the paper and his extensive influence in the local militia unit, which he had joined at age twelve, as the base for launching a remarkably successful career as a soldier-politician and member of Parliament. First elected in 1892, Hughes retained his seat in every federal election until his death in 1921. Haycock persuasively argues that Hughes's finely honed skills as a practitioner of constituency politics—adherence to Protestant values, party loyalty, vigorous partisanship, extensive use of patronage, and unquestioning faith in the voluntary active militia—account at once for his electoral success and for his ultimate failure as a minister of the crown during the Great War.

Hughes was a controversial man, intensely ambitious, egotistical, and frequently outrageous in his defense of his beliefs and his actions. French Canadians were deeply offended by his virulent anti-Catholicism. Professional soldiers in Canada's Permanent Force and in the British forces in South Africa, where Hughes briefly served in the South African War, and in the Great War bristled at his contemptuous dismissal of the professionalism, discipline, and order of the regular soldier. Party members and, after 1911 when he became minister of militia and defense, cabinet colleagues resented his flagrant self-promotion and his haphazard administration of his department.

Properly, nearly half of Haycock's book is devoted to the highlight of Hughes's career, his work as minister during the Great War. Careful attention is given to each of Hughes's major responsibilities: voluntary raising of recruits for the Canadian Expeditionary Force (CEF), letting contracts and securing supplies for the CEF and for the armies of Canada's allies, and overseeing the administration of the Canadian forces in training in Britain and fighting at the front. Haycock argues that Hughes's innovative approaches to recruiting and supply procurement were initially sound in principle but soon flawed in practice by Hughes's failure to understand that professionalism and sound administration were essential to the prosecution of a war of long duration. And from the beginning the oversight of the overseas forces was confused and chaotic. In the end Hughes's "regime collapsed before the demands of total war" (p. 320). By late 1916 the government of Canada could no longer tolerate his excesses, and in November Sir Robert Borden, the prime minister, dismissed him from the cabinet. "And so," Haycock concludes, "he was ruined by the war itself" (p. 318).

Haycock's analysis of Hughes's career is balanced, detailed, sympathetic, and thoughtful. This is an important book. It should be required reading for students of Canadian political history and of the military history of Canada and Britain in the Great War.

ROBERT CRAIG BROWN
University of Toronto

NORMAN BUCHIGNANI *et al. Continuous Journey: A Social History of South Asians in Canada*. (History of Canada's Peoples; Generations.) Toronto: McClelland and Stewart or Ministry of Supply and Services. 1985. Pp. viii, 250.

In the mid-1970s the Canadian government, as part of its policy of multiculturalism, began commissioning histories of ethnic groups. In this volume of the "Generations" series Norman Buchignani and two other anthropologists study Canadians whose origins were in India, Pakistan, Bangladesh, and Sri Lanka. The historical sections draw extensively on Canadian government records; the contemporary chapters are based on a number of theses and other studies and, although not explicitly documented, on fieldwork.

The title, *Continuous Journey*, comes from a 1908 immigration law that required immigrants to come to Canada by a through-ticket from their country of origin. Although India was not specifically mentioned, the regulation applied chiefly to its people and effectively checked immigration. Indeed, at its peak, Canada's pre-1914 South Asian population was probably less than twenty-five hundred and was remarkably homogeneous, consisting mainly of male Sikhs from the Punjab who settled in British Columbia. The immigration barriers deterred family formation and provoked great resentment, which Indian nationalists exploited.

For the post–World War II era "Continuous Journey" is a misnomer. Relaxed immigration laws allowed South Asians to come from the Caribbean, eastern Africa, and Fiji as well as the Indian subcontinent and to increase their number in Canada from about two thousand in 1951 to the authors' estimate of three hundred ten thousand in 1982. As well as having diverse geographical origins, the more recent immigrants represent over a dozen languages, three major religions, various cultural traditions, and assorted occupational backgrounds. Unlike the

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early immigrants, the later immigrants have settled in almost every part of Canada. Their consciousness of being South Asian has been largely a "made-in-Canada" phenomenon, a response to stereotypes and popular prejudices in the larger community. The authors treat that subject fairly and sensitively but downplay internal tensions that occasionally erupt in well-publicized violence.

The most valuable parts of the book are its examination of the importance of family, its survey of community institutions, and its speculation on the acculturation of the second generation. Despite difficulty in singling out individuals, it is disappointing that outside of the historical chapters the authors lose sight of personalities. Skillfully presented biographical sketches can enliven a study without making it a collective hagiography. The volume, however, is a worthy member of the "Generations" series.

PATRICIA E. ROY
University of Victoria

LATIN AMERICA

CHARLES R. CUTTER. *The Protector de Indios in Colonial New Mexico, 1659–1821*. Albuquerque: University of New Mexico Press, with the cooperation of the Historical Society of New Mexico. 1986. Pp. xii, 129. Cloth $17.50, paper $8.95.

Piecing together fragments of information, most of it from archival sources, Charles R. Cutter describes and evaluates the role of the protector of Indians in a peripheral area of the Spanish empire. Spanish law required the *protector de indios* to "aid and defend" the natives, both in and out of the courtroom. Although the *protector* functioned sporadically in New Mexico, Cutter makes clear that this official, together with the general climate of Spanish law, gave the Pueblo Indians of New Mexico effective access to the Spanish legal system.

Cutter corrects the misconception that the office of *protector de indios* did not function in seventeenth-century New Mexico, but he argues that in that century the *protector* and other officials as well failed to safeguard the legal rights of the Pueblos. This failure, he suggests, contributed to the revolt of the Pueblos in 1680—one of the most successful Indian rebellions in the Spanish empire. With the reassertion of Spanish control over the Pueblos in the eighteenth century, Cutter sees two important changes. First, Spaniards took greater pains to protect Indian rights in New Mexico as they sought to avoid another catastrophic rebellion. Second, Pueblos learned to operate Spanish legal machinery, bringing it to bear with special vigor when their lands came under attack. Cutter cites numerous examples of Pueblos who, with or without the aid of the usually unfilled office of *protector*, successfully used Spanish legal mechanisms to protect their rights under Spanish law. He finds in the Spanish colonial period the roots of the process that ensured the survival of "the communal quality of Pueblo culture that has persisted to this day" (p. 79).

Although Cutter notes that Indians in the English and French empires had little access to the legal system and lacked an official comparable to the *protector* (a judgment he might have tempered had Yasuhide Kawashima's *Puritan Justice and the Indian* [1986] been available to him), he avoids the Hispanophile romanticization that has characterized much writing on the Borderlands. He critically examines the implementation of Spanish law as well as its intent and draws comparisons between the way the protector of Indians functioned in this peripheral province and in the core areas of New Spain. This is a useful companion to Woodrow Borah's *Justice by Insurance: The General Indian Court of Colonial Mexico* (1983) and an admirable study, diligently researched and clearly written.

DAVID J. WEBER
Southern Methodist University

P. MICHAEL MCKINLEY. *Pre-Revolutionary Caracas: Politics, Economy, and Society, 1777–1811*. (Cambridge Latin American Studies, number 56.) New York: Cambridge University Press. 1985. Pp. xiv, 245. $39.50.

P. Michael McKinley's book is the first English-language monograph to appear on colonial Venezuela in over ten years and is the first in any language to provide a broad synthesis of the late colonial period. As such, it is a well-researched study that makes a valuable contribution to understanding the province from its administrative centralization in 1777 until the start of independence. The main thesis is that Caracas experienced a remarkable period of economic prosperity accompanied by political stability and social cohesion that lasted until the Napoleonic invasion of Spain shattered the traditional relationship between colony and empire. Once monarchic authority dissolved, the Caracas elite—prosperous, unified, and confident—assumed power from royal officials in a peaceful takeover. The independence movement later became violent because "key individuals, reacting to the political exigencies of the moment, cold-bloodedly chose the use of extreme violence to break the stability of the colonial order" (p. 2).

McKinley ably demonstrates the lack of conflict among the *caraqueño* elite by studying a group of wealthy creoles, Spaniards, landowners, and mer-

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chants. The wills of over eight hundred individuals are skillfully used to analyze the accumulation of wealth during the late 1700s and to demonstrate the close personal and economic ties that existed among the elite. The interpretation once generally accepted that hacendados and merchants were opponents is dispelled when the author shows that many individuals functioned in both capacities and were rarely indebted to one another, a fact not observed in many other parts of the empire. Rising prices for Venezuela's agricultural commodities, crop diversification, and few restrictions on trade outside the confines of the empire allowed for an unprecedented period of growth, which made the late eighteenth century a "golden age of economic expansion" (p. 5).

Turning to politics, the author emphasizes the overwhelming importance of the Spaniards who filled the new and powerful post of intendent. The ability of these men to identify with the creole elite and to soften imperial policy where antithetical to local interests permitted Spaniard and creole to work in an atmosphere of mutual respect and cooperation. In this regard, the author hopes to end once and for all the argument that the two groups opposed one another. How, then, can McKinley explain that such a harmonious and peaceful colony within a few years disintegrated into a fragmented society torn apart by racial conflict and civil war? The author, after examining the early stages of independence, concludes that blame must rest with Simón Bolivar and the royalist José Tomás Boves, who unduly agitated the masses and employed any method to achieve their goals.

This view of Bolivar will not make McKinley popular in Venezuela, but it also underestimates the racial tension, slave resistance, unrest in the llanos, urban agitation, poverty of the lower classes, foreign influences, and interprovincial rivalries that prevailed throughout the colonial period. Had McKinley focused as much on these points as he did on the elites, his explanation of the fratricidal wars might be different. Nevertheless, the author provides much new and useful information on late colonial Caracas, its economy, and its society.

KATHY WALDRON
Citibank N.A.
New York, New York

LOWELL GUDMUNDSON. *Costa Rica before Coffee: Society and Economy on the Eve of the Export Boom.* Baton Rouge: Louisiana State University Press. 1986. Pp. xvi, 204. $30.00.

The myth of Costa Rican egalitarianism is one of the most cherished concepts among Costa Rican nationals and Latin Americanists generally. It has been both popular and persuasive to explain Costa Rica's unique political success in terms of a rural democratic tradition dating from colonial times. In the present book Lowell Gudmundson challenges this tradition in a highly convincing manner. Focusing on the 1840s, when coffee cultivation and exports began to transform Costa Rican society along capitalist lines, Gudmundson's work has relevance for the entire span of modern Costa Rican history.

After a thorough explanation and historiographical analysis of the myth of Costa Rican egalitarianism, Gudmundson offers a perceptive review of colonial socioeconomic development and the rise of coffee exporting in Costa Rica. His discussion of the historiography is especially valuable, for it demonstrates the relationship between the various interpretations of Costa Rican history and the development of social democracy in the country. He then argues that rural Costa Rica before coffee was based on nucleated rather than dispersed settlement and that there were clear differences of wealth and status, with significant social inequality. Gudmundson documents the existence of a precapitalist elite and analyzes the multiplicity of its roles and relationships with the peasantry, before and during the transition to coffee cultivation. He incorporates a detailed discussion, amply documented, of the household structure, sex roles, and inheritance patterns in precoffee Costa Rica, concluding that mid-nineteenth-century Costa Rica "presented significant differentiation along social and spatial lines inherited from colonial times." Moreover, he continues, "a certain flexibility within sex roles, particularly for women in the larger villages and cities, belies any simple idea of a patriarchal household structure or social order" (p. 22). A chapter on demographic and geographic variables reinforces Gudmundson's contention that Costa Rican traditional society was far from egalitarian. Qualitatively and quantitatively, Gudmundson thoroughly dispels the mythical model "whose grain of truth can hardly be denied, but the statement of which often falsifies equally important truths and always misleads its contemporary advocates" (p. 24).

In the process of describing precoffee Costa Rica, Gudmundson reveals the depth and breadth of his research as he details the transition to agrarian capitalism, noting important differences from other Central American states and some similarities to Colombia. He also challenges the idea that Costa Ricans were almost purely European in origin, noting strong mestizo features in early nineteenth-century elites and the "whitening" of those elites that occurred with the immigration of Northern Europeans as coffee became dominant.

If Costa Rica was not a rural egalitarian society before coffee, then what is the origin of the democratic myth that has been so powerful a force in

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Costa Rican social and political history? Gudmundson does not evade that question. In his conclusion he argues that the rural egalitarian myth is the product of the coffee culture, rather than its producer, and it has been used to support the capitalist, agroexport order that supplanted colonial society. Although he acknowledges that it was perhaps "built upon important but isolated elements inherited from the pre-coffee order . . . it was certainly created by bourgeois society at all levels . . . the product of the coffee boom" (p. 161). Thus, the creation of the myth, Gudmundson leads us to believe, contributed not only to Costa Rica's successful and relatively painless transition from a subsistence to an agroexport economy but eventually to Latin America's most successful social democracy.

Meticulous scholarship characterizes this solid monograph, which is enhanced by the numerous graphs, charts, tables, and informative appendixes that illustrate the massive statistical data Gudmundson has assembled. This sort of research does not lend itself to especially scintillating writing, but the serious reader will find clarity and precision in this model of social scientific history.

RALPH LEE WOODWARD, JR.
Tulane University

ALLEN WELLS. *Yucatán's Gilded Age: Haciendas, Henequen, and International Harvester, 1860–1915.* Albuquerque: University of New Mexico Press. 1985. Pp. xiii, 239. $27.00.

In recent years Mexican regional history has been the focus of a number of rancorous scholarly debates, often testing once-sacrosanct views of the older, nationally oriented, history. Among the most hotly contested issues has been the role of the International Harvester Company (IHC) in the henequen economy of Yucatán during the "guilded age" of Porfirio Díaz. Allen Wells's contributions to this controversy are well known and are here republished along with two new essays—one on the failure of the henequen boom to stimulate the peninsula economy as a whole and the other a case study of the transformation of a traditional hacienda into a commercial henequen plantation.

In his introduction Wells describes the state's prehenequen economy. Hacienda and village coexisted as subsistence producers until eighteenth-century market changes and postindependence liberal legislation encouraged elite encroachment on Mayan lands. Local conflicts erupted in 1847 in the bloody Caste War. One long-term effect of the war was to provide the victorious *hacendados* with a statewide system of coercion and control so effective in repressing protests by villagers that, when the rest of Mexico exploded in revolution in 1910, Yucatán's demoralized Maya remained quiescent. Wells argues that the Caste War did not spur the henequen economy, as some historians maintain. Rather, it encouraged migration to the more peaceful northwest. There settlers encountered a poor, limestone-ridden soil particularly well suited for henequen, a fibrous plant traditionally woven into cordage. Hence, in the 1880s, when North American farmers demanded a cheap binder twine strong enough to withstand the rigors of the new mechanical harvesters, the Yucatán elite was in the right place to respond. By the turn of the century, northwestern planters were supplying sufficient raw materials to manufacture 85 percent of all North American binder twine.

Wells describes and analyzes the process by which local planters became well-paid "collaborators" with the IHC. He contends that IHC's control of raw materials through their subordination of Yucatán producers and their monopoly of the sale of binder twine in the United States were the key factors in depressing henequen prices, not macroeconomic variables as argued by Wells's critics. Disappointingly, Wells chose not to include in his book his detailed response to those critics. (See Wells and Gilbert M. Joseph's "Collaboration and Information Empire in Yucatan: The Case for Political Economy" *Latin American Research Review* 18, 3 [1983]: 204–18.) The reader might also wish to consult Wells's three previously published articles because they contain useful introductory and historiographic discussions that the author chose not to include in his monograph.

In his several chapters on the elite's business dealings, Wells portrays the planters as many-sided individuals and treats their risk-taking talents far more sympathetically than usually has been the case among modern-day historians. Yet the contradiction inherent in monocrop production forces Wells to conclude that as a class the henequen planters did not create an economy beneficial to the entire state. Their great wealth too often served only the narrow interests of henequen production at the expense of regional development, and vast sums were wasted on poor investments or lavished on "status aspirations."

In his chapter on labor Wells attempts "to correct the exaggerated misconceptions of both apologists and critics" (p. 152) of the planters, singling out contemporary critic John Kenneth Turner's *Barbarous Mexico*. Wells denies, for example, that planter paternalism mitigated the worst aspects of the system, as apologists have long alleged, and he disputes the commonly held view of both sides that Yucatán peasants docilely accepted their fate. Yet Wells's descriptions of exploited workers laboring under wretched conditions bears strong resemblance to those pathetic wrecks of human beings Turner saw

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with his own eyes. Wells explains better what Turner saw but does not refute his vision.

Wells's research is built on enormously broad and varied sources and is much fuller and more satisfying than the narrowly focused economic history of his critics. Although his regional concentration suffers from a certain lack of national perspective, Wells nonetheless provides us with a very solid, thoughtful contribution to the construction of a truly national history of Mexico.

RODNEY D. ANDERSON
Florida State University

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

BERNARD P. DAUENHAUER, editor. *At the Nexus of Philosophy and History*. Athens: University of Georgia Press. 1987. Pp. xix, 222. $27.50.

BERNARD P. DAUENHAUER, Introduction. RICHARD BERNSTEIN, History, Philosophy, and the Question of Relativism. LOUIS DUPRÉ, Philosophy and Its History. RICHARD DIEN WINFIELD, Can Philosophy Have a Rational History? BOWMAN CLARKE, Logic and History. NANCY STREUVER, Philosophical Problems and Historical Solutions. OFELIA SCHUTTE, The Place of History in Nietzsche's Thought. KIRK WILLIS, Bertrand Russell on History: The Theory and Practice of a Moral Science. ERNST BREISACH, Two New Histories: An Exploratory Comparison. BERNARD P. DAUENHAUER, History's Point and Subject Matter: A Proposal. THOMAS R. FLYNN, Michel Foucault and the Career of the Historical Event. A. ANTHONY SMITH, Habermas and History: The Institutionalization of Discourse as Historical Project.

ALEXANDER J. FIELD, editor. *The Future of Economic History*. (Recent Economic Thought Series.) Boston: Kluwer-Nijhoff. 1987. Pp. ix, 356. $54.95.

ALEXANDER J. FIELD, The Future of Economic History. BARRY EICHENGREEN, Macroeconomics and History. DAVID FEENY, The Exploration of Economic Change: The Contribution of Economic History to Development Economics. MARK THOMAS, General Equilibrium Models and Research in Economic History. MICHAEL HAINES, Economic History and Historical Demography: Past, Present, and Future. WILLIAM LAZONICK, Theory and History in Marxian Economics. GAVIN WRIGHT, Labor History and Labor Economics.

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FRED M. HAYWARD, editor. *Elections in Independent Africa*. (Westview Special Studies on Africa.) Boulder, Colo.: Westview. 1987. Pp. xvii, 318. $23.85.

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ARTICLES

TO THE EDITOR:

James Axtell's comment on Europeans, Indians, and the Age of Discovery in the *AHR* of June 1987 is, in my judgment, one of the most valuable and useful pieces we have had to help correct the persistent errors that appear in textbooks, especially mistakes and downright racist characterizations about Indians. Some years ago, the Indian Historical Society in San Francisco published an excellent book by Jeanette Henry and Rupert Costo, *Textbooks and the American Indian* (San Francisco, 1970), focusing entirely on misconceptions of Indians that appeared in popular textbooks written by Merle Curti, Samuel Eliot Morison, and others. Because Henry and Costo showed how Indians in textbooks have been maligned and misunderstood, not only in the Age of Discovery but throughout all of American history, their work should not be overlooked.

On one point, I disagree with Axtell's penetrating critique of textbooks. His criticism of Bernard Bailyn's text as an example of carrying on the Black Legend of a brutal Spanish Indian policy may not be altogether merited. I found, in my recent study of Franciscan Indian policy and Father Junipero Serra, that the antecedents of Franciscan missionary policy reached back into the conquest period and that the Black Legend is in many respects an accurate interpretation of history. Although the Dominican missionary and historian Bartolomé de Las Casas heavily censored Spanish brutality in the conquest, arguing that Indian peoples had a culture and a lifestyle that should be treated with respect, it was the conquistador mentality that prevailed in policy to justify savage treatment of Indian people. The Spanish Franciscan missionaries were a part of the expansion of Castilian society and a classic kind of conquest practiced first by the Moors, but later by the Spaniards in "just war," a bitter prolonged struggle for the peninsula.

Juan Ginés de Supúlveda, Spanish scholar and intellectual warrior of the "just war," utilized Aristotle's theory of natural slavery to eulogize Spanish virtues and contrast them with Indians. To justify force against them, he classified Indians as a people with stunted minds. They could be compared with monkeys and creatures of inversion (witches and other deviants) who had skewed biological origins. Leading Franciscans followed Supúlveda's teachings and criticized Las Casas as a troublemaker. One of their number, Fray Toribio de Motolinia, in 1555, bitterly attacked Las Casas and praised Hernan Cortez as the God-sent leader of Christianity. The Conquest itself, and the accompanying phenomena, was, he argued, an expression of divine will.

Indeed, the "just war" mind-set of Franciscans in their treatment of Indians is well expressed by Saint Francis Solano (1549–1610), missionary among South American Indians for twenty-one years, who set a precedent for flogging native people. He unhesitatingly ordered that "his Indians be whipped by his *fiscales*." In justifying harsh policies that persisted in California as late as the eighteenth century, Father Serra cited Solano as well as a cleric of Peru, Alonso de las Pena Montenegro (1653–87), Bishop of Quito, who maintained that "barbaric" Indians should be coerced by "burning their homes" as "just punishment."

The recent scholarship relating to the "just war" and Spanish policies by James Turner Johnson, Anthony Pagden, and others, revises revisions of the Black Legend. The evidence is that the humanistic policies advocated by Las Casas, as well as other leading Spanish writers, were all but nullified by the victory of the "developers" of the New World who used the argument for converting pagans as a justification for a resort to force, brutality, and exploitation. The Conquest was a "just war" and human sacrifices by Indians justified, as the Franciscans saw it, their attempt to obliterate the culture of native people. Very probably, those adjectives used by Bailyn in characterizing the conquering Spaniards of the Age of Discovery have the ring of truth. Some of those same adjectives may be applied to the French and English as well, but we may recognize in the Jesuit missionaries of the New World a most enlightened and sympathetic understanding of Indian people as symbolized by their most eloquent spokesman, Father Joseph-Francois Lafitau.

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TO THE EDITOR:

James Axtell's "Notes and Comment" critique of textbooks makes valuable reading for their authors, although it is familiar fare for us to receive the advice from a colonialist that we must double the space given to the Age of Discovery. Publishers routinely solicit confidential reviews of textbooks, and one thing we can always count on is that each critic will insist that not enough space is given to his or her area of specialization. They may of course be right; this is always the question to resolve.

Perhaps some comments on the realities of textbook writing will help. We have very little space, perhaps a thousand printed pages, in which to tell this huge story—the whole of the American past—and we cannot go significantly beyond that fairly common size, or no one will use the book. Hence, whenever something new goes in, something else goes out. That is the iron law of revising textbooks. Thus, in this case, if we double the space given the Age of Discovery, what is reduced or eliminated? The treatment of the Constitution, of women and the family, of the Progressive Era, of the war in Vietnam? Each subject in a given edition is there because it has already survived a bitter struggle for its space against all comers. It is a tremendous, difficult, often agonizing task, cutting and paring and squeezing to get everything in that the author is convinced must be there.

Another reality: each year, new books and articles on U.S. history appear, by (literally) the hundreds. It is awesome, to a textbook author even a bit threatening, to watch the annual outpouring. It is like watching a non-stop, monster Fourth of July fireworks display, more and more fine works bursting in the historical firmament, tumbling up into the sky one after the other in a steady stream. There is never a moment of stillness.

How does the Age of Discovery fare in all this? Does it display a "vast and turbulent historiography," as Axtell writes? Actually, by my own count in two recent issues of the three journals that review the largest number of U.S. history books, I note that out of almost 400 book reviews, only a dozen were on books in the fields Axtell lists: the Age of Discovery, Indians in colonial America, and the history of the Spanish and French empires. He criticizes us for inadequate attention to these topics, but the profession itself is not as actively interested in those subjects as U.S. history textbooks already are. If the truth be known, the Age of Discovery is much more prominent in the textbooks than it is in what is being published these days.

The fact is that our collective sense, within the discipline, of what is important to talk about in American history has changed remarkably. And the author or authors of what is intended to be an up-to-date and comprehensive history of the American people and nation simply must reflect in the body of the book they write that changed conception of our shared past.

Axtell repeatedly points in disapproval to the fact that textbooks routinely divide at Reconstruction, so that the last century is given far more space than the several centuries before, which he calls "pedagogical nonsense," and "egregious whiggism." Unfortunately for his argument, time is not an absolute but a relative commodity. Recent history is simply far more complex. Change accelerates, formerly small populations become gigantic, ways of life for whole societies are massively changed, life cycles shift astonishingly, the advent of democratic politics renders that arena vastly more complicated, the life of the mind goes through great transformations. In actual truth, what we need is some mechanism in the profession by which the dividing point can be moved up from Reconstruction (where it has been since at least 1945) to at least the 1890s. We are a century, now, from the late 1880s, not (as in 1945), the mid-1840s. And the last forty years have been enormously complex.

There are a great many fine textbooks presently available. Each time I pick one up, unlike Axtell, I am usually much impressed with the scholarly pains and often gifted narrative skills that have gone into them. Try writing one; it is not easy. All of these books share the same weighty task: telling the whole story of America and its people; going out on a limb and saying *this* is the American nation, *this* is what is important to know about that vast country, if we are going to understand it historically. No other scholars do this, save a few public historians who write for a living. It is a rare intellectual undertaking; out of the thousands of historians in this country, perhaps fifty have been involved in the writing of the sixteen textbooks that Axtell critiqued, which he terms the "most popular ones" currently in use. None of those authors is insensible to the importance of their task. With potentially thousands of students before them, as well as hundreds of professors, they approach their task (if my own acquaintance with many of them is reliable) with high seriousness.

For this reason, we need criticism, and welcome the decision of the *American Historical Review* to publish Axtell's remarks. What we need, however, are criticisms that do more than look at the first two chapters, and say, in isolation from the rest of the book, "double the space given to ——" (fill in the blank with the critic's specialization). We need reviews of entire books, which examine our overall purpose and structure in them, and decide whether, within that purpose, we have done well or poorly.

ROBERT KELLEY
University of California, Santa Barbara

PROFESSOR AXTELL REPLIES:

While I am grateful for Wilbur Jacobs's and Robert Kelley's appreciation of my admittedly personal

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critique of textbooks, I cannot accept their particular caveats.

I cannot agree with Jacobs that the Black Legend has any viability as an *interpretation* of Spanish colonial history, an issue that was thoroughly aired by Lewis Hanke and Benjamin Keen in the pages of the *Hispanic American Historical Review* nearly twenty years ago (49 [1969]: 703–19, 51 [1971]: 112–27, 336–55). First, it reduces the Spanish empire to its brief *conquest* phases, particularly in Mexico. This not only ignores the rest of some three centuries of colonial history; it also conveniently forgets that the majority of conquistadors had no military experience but represented a cross-section of ordinary Spanish settlers (see James Lockhart, *The Men of Cajamarca*, [Austin, Tx., 1972] and Mario Góngora, *Studies in the Colonial History of Spanish America* [Cambridge, 1975], chap. 1) and that imported diseases, not warfare or ill-usage, killed the vast majority of Latin American natives.

Second, in dwelling solely on the terrible human costs of conquest, the legend ignores the considerable achievements of the Spanish empire, which was more extensive and lasted longer than Rome's. It fails to tell the less sanguinary story of the large folk emigration from Spain and its dominions and of the (re-) settlement, bureaucratic governance, and Christianization of vast new territories and populations (see John H. Elliott, "The Iberian Achievement," *Princeton University Library Chronicle*, 47 [1985]: 25–47).

And, finally, the legend is myopically Eurocentric: it reduces the Indians to passive victims of Spanish aggression, without any hint that native allies were indispensable to the greatly outnumbered conquistadors or that Indian individuals and groups continued to make creative adaptations to the Hispanic presence throughout the colonial period in order to maintain maximal reservoirs of sovereignty and cultural autonomy (see Nathan Wachtel, "The Indian and the Spanish Conquest," in Leslie Bethell, ed., *Cambridge History of Latin America, I: Colonial Latin America* (Cambridge, 1984), chap. 7. None of this is meant to deny the essential truth of the Black Legend as it pertains to the Spanish treatment of Indian peoples; it is only to insist that its kernel of truth be placed in a larger, more balanced context.

As for Kelley's impassioned defense of the status quo, I am hard put to deal with appeals to the textbook authors' "high seriousness" and the "iron laws" of textbook publishing without sounding like an ungrateful scofflaw. (While snappy, comments such as "Try writing one" are not relevant responses to one's critics.) Let me suggest that Kelley's "iron laws" are nothing but received pieties based on unexamined assumptions and habits. Is it true that (1) textbooks are as necessary to higher history education as they are in the United States (other civilized countries seem to get along with far fewer or with none at all); (2) successful textbooks must be no longer than a thousand pages (in its heyday, Morison and Commager's *The Growth of the American Republic* ran to 1,800 single-columned pages); and (3) textbooks must be divided at Reconstruction—or the 1890s (God forbid!)—because recent history is more "complex" than earlier history? Merely asking such simple questions should suggest several viable alternatives to the discouraging status quo.

And how do I handle the countercharge that every critic wants the textbook treatment of his specialty doubled? Only by reiterating the case for the "Age of Discovery" on its own merits: it is the *formative* period of American history and therefore indispensable for understanding what follows (and most of what follows the colonial period is easily predictable by anyone who knows the former); it is a *long* piece of undeniably American history; and it is *complex* beyond the wildest imaginations of recent American historians who have consistently ignored it in their graduate education and subsequent reading and teaching. Unfortunately, such historians cannot fathom the depth or breadth of its vigorous historiography by looking at six issues of journals that review *U.S.* history books: ethnohistory, the sixteenth century, and the histories of New Spain and New France are all notoriously neglected by such nationalistic bibliographies, for the same reasons they are neglected by the textbooks themselves.

The heart of Kelley's defense is that "recent history is simply far more complex" than that of earlier centuries and therefore deserves proportionately more space in the texts. Is this true? Is the recent past's greater "complexity" real or only apparent, the result of greater *quantities* of documents spawned by faceless bureaucracies, which in turn generate greater *numbers* of dissertations and monographs by historians seeking reasonably familiar and accessible research topics? It can be argued, on the contrary, that American culture in the twentieth century has become increasingly homogenized and *less* complex because of national and transnational institutions and communication media, that regionalism has become *less* pronounced, and that government has been increasingly *centralized* in Washington, D.C.

By contrast, the sixteenth century—which witnessed the Reformation and Counter-Reformation, the European investment of "new" continents, the inauguration of a global economy, and some of the most momentous intellectual changes in history—seems no less "complex" than the twentieth and equally deserving of our understanding and appreciation. And other centuries before our own have perhaps equal claim. If the Age of Discovery seems less complex or less important to us and our young students, with their extremely foreshortened social memories, perhaps we are simply uninformed and continue to be hoodwinked by our textbooks and course outlines, which reduce that fascinating and

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fundamental period to numbing cliches and airy generalities.

JAMES AXTELL
College of William and Mary

REVIEWS OF BOOKS

TO THE EDITOR:

I am pleased that Gary M. Hamburg finds my book, *Nobility and Privilege in Late Imperial Russia* (*AHR*, 92 [1987]: 1006–07) "valuable as a social history" but disappointed that he deems its political analysis "curious and unconvincing." Most of all, however, I regret his failure to place my book in the context of recent Western historiography on the Russian nobility—specifically, his own *Politics of the Russian Nobility, 1881–1905* (1984) and Roberta T. Manning's *The Crisis of the Old Order in Russian: Gentry and Government* (1982).

In his book, Hamburg argues that the nobility's politics was a politics of economic interest and largely a response to the long depression in grain prices in the last quarter of the nineteenth century. He attributes the nobility's political failure to their lack of unity, which explains their inability to produce a coherent ideology and program. I differ from Hamburg in treating the politics of the "noble question" as an attempt by traditionalists to defend a particular moral order (society envisioned as a hierarchy of social estates and firmly based on privilege) against the forces of change set in motion by the great reforms. The traditionalists lacked neither a coherent ideology nor a program—only effective support from the state and sufficient backing from the nobles themselves. It is the latter point—the indifference on the part of the overwhelming majority of nobles to the program of their self-proclaimed spokesmen—that is a major theme of my chapters 3 to 7. Hamburg claims that "these chapters sit uneasily with the first section of the book." But the nobility's massive indifference to the traditionalists' program is explicable only with reference to the first estate's largely successful accommodation to a changing economic environment—the subject of the first section. I am gratified that Hamburg finds my analysis of this accommodation "clear and convincing." He fails, however, to note that this analysis is, *inter alia*, a refutation of one of his book's principal theses, namely, that the depression in grain prices is the key to understanding the economics and politics of noble landownership.

My book also takes issue with Manning's notions of an economically and politically resurgent nobility in the early twentieth century and of an intransigent nobility as the principal agent of the Old Regime's downfall. How convincing a case I have made, only the readers of my book can decide. It is the office of the reviewer, apart from bestowing praise and criticism, to make clear to prospective readers the nature of the basic points at issue in a scholarly controversy.

SEYMOUR BECKER
Rutgers University

PROFESSOR HAMBURG REPLIES:

Seymour Becker is correct that an "office of the reviewer, apart from bestowing praise and criticism, [is] to make clear to prospective readers the nature of the basic points at issue in a scholarly controversy." Sometimes, however, a reviewer simply lacks the space to execute this task. Becker's valuable book, *Nobility and Privilege in Late Imperial Russia*, takes issue not only with previous Western scholarship on this subject (by me and Roberta Manning) but with Soviet historiography on the nobility as well. In the several hundred words allotted for my review, I found it impossible conscientiously to describe Becker's many disagreements with every other scholar who has worked in the field; instead, I thought it fairer to Becker and prospective readers to outline the major themes of Becker's book, praise his social portrait of the nobility, and raise a few questions about his political analysis of the late Imperial regime. I thought it inappropriate to make a short review of Becker's book into a vehicle for elaborating my own disagreements with him about the fate of the nobility, let alone a comparison of his few words about the post-1905 period with Manning's long, magisterial book on that subject. Polemical reviews usually work to the disadvantage of the author whose book is under review, and their chief effect is often to obscure the author's major themes and contribution to scholarship. If my review, whatever its deficiencies, persuades prospective readers that they should attentively study Becker's book, then my decisions concerning what needed to be included in and excluded from the review will be vindicated. If not, my apologies to Professor Becker will be in order.

GARY M. HAMBURG
University of Notre Dame

TO THE EDITOR:

I am writing in response to the highly politicized nature of William Monter's review of my work, *The Conquest of Poverty* (*AHR*, 92 [1987]: 967–68). Unable to challenge the main argument or the evidence in my study, Monter warns readers off the book because he has found some typographical and proofreading errors and notably because it is Marxist in its language and approach. Toward the close of his review, he even goes so far as to say "an East German could not have said it better." Monter's remark, one can only hope, is in reference to the East German school of Reformation studies. As students of the Reformation such as Professor Monter ought to know, these historians have made a significant contribution to Reformation research in recent years.

HENRY HELLER
University of Manitoba

Professor Monter declines to respond.

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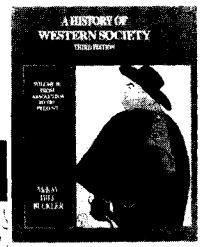
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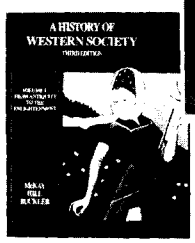
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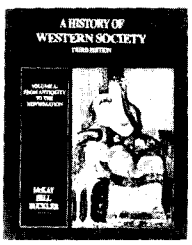
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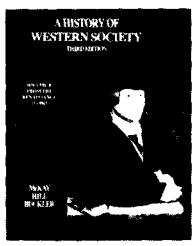


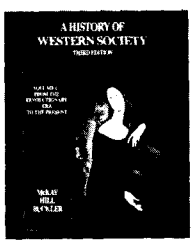




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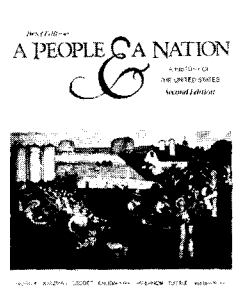
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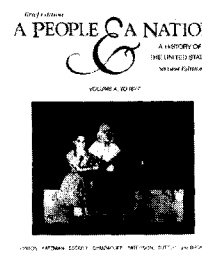
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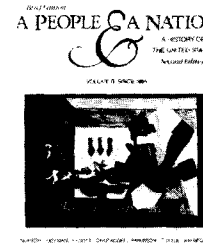
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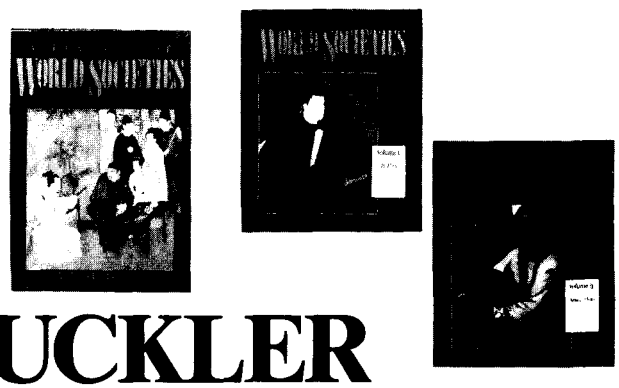
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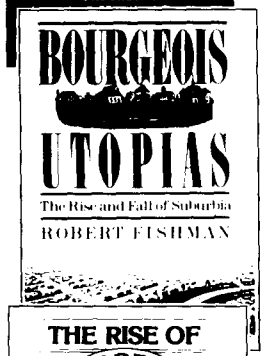


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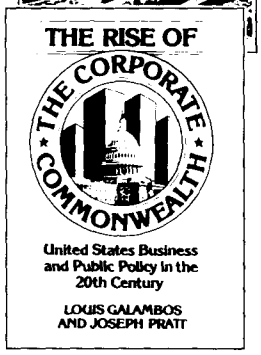


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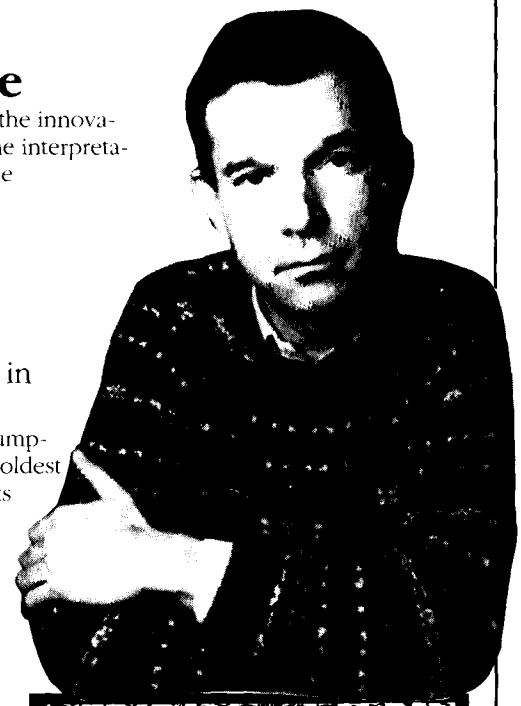
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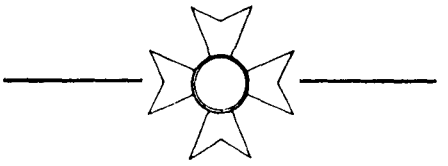
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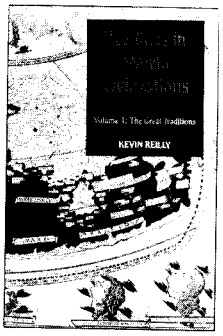
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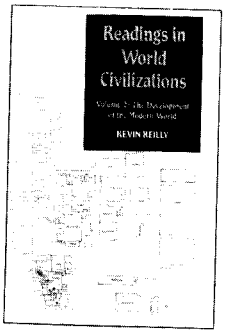
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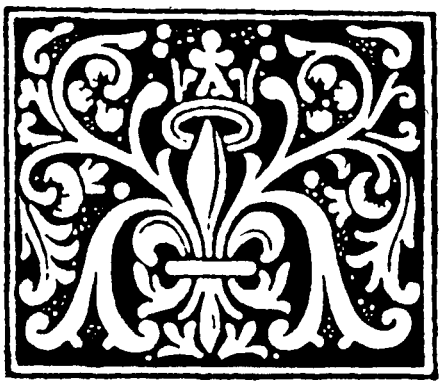
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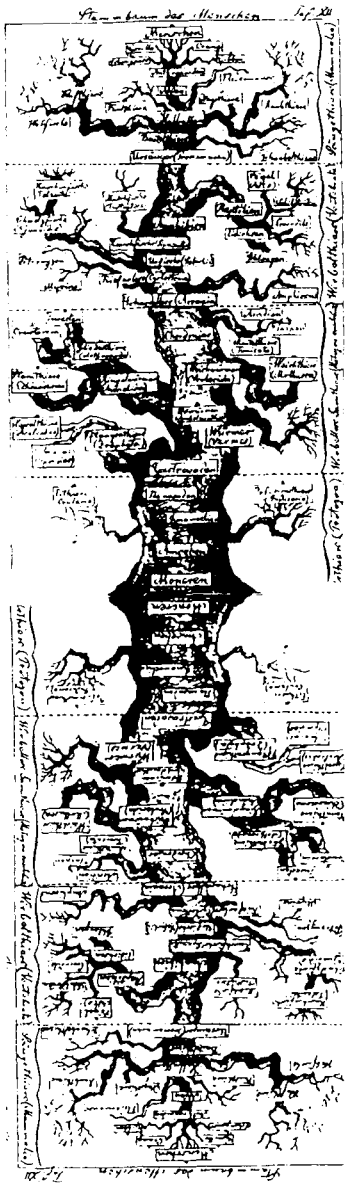
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